

# Psychological Review

RICHARD L. SOLOMON, Editor  
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# THE PSYCHOLOGICAL REVIEW

## ON THE POSSIBLE PSYCHOPHYSICAL LAWS<sup>1</sup>

R. DUNCAN LUCE

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This paper is concerned with the century-old effort to determine the functional relations that hold between subjective continua and the physical continua that are presumed to underlie them. The first, and easily the most influential, attempt to specify the possible relations was made by Fechner. It rests upon empirical knowledge of how discrimination varies with intensity along the physical continuum and upon the assumption that jnd's are subjectively equal throughout the continuum. When, for example, discrimination is proportional to intensity (Weber's law), Fechner claimed that the equal-jnd assumption leads to a logarithmic relation (Fechner's law).

This idea has always been subject to controversy, but recent attacks upon it have been particularly severe. At the theoretical level, Luce and Edwards

(1958) have pointed out that Fechner's mathematical reasoning was not sound. Among other things, his assumption is not sufficient to generate an interval scale. By recasting his problem somewhat—essentially by replacing the equal-jnd assumption with the somewhat stronger condition that "equally often noticed differences are equal, except when always or never noticed"—they were able to show that an interval scale results, and to present a mathematical expression for it. Their work has no practical import when Weber's law, or its linear generalization  $\Delta x = ax + b$ , is true, because the logarithm is still the solution, but their jnd scale differs from Fechner's integral when Weber's law is replaced by some other function relating stimulus jnd's to intensity.

At the empirical level, Stevens (1956, 1957) has argued that jnd's are unequal in subjective size on intensive, or what he calls prothetic, continua—a contention supported by considerable data—and that the relation between the subjective and physical continua is the power function  $ax^b$ , not the logarithm. Using such "direct" methods as magnitude estimation and ratio production, he and others (Stevens: 1956, 1957; Stevens & Galanter, 1957) have accumulated considerable evidence to but-

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Ward Edwards, E. H. Galanter, Frederick Mosteller, Frank Restle, S. S. Stevens, and Warren Torgerson have kindly given me their thoughtful comments on drafts of this paper, many of which are incorporated into this version. I am particularly indebted to S. S. Stevens for his very detailed substantive and stylistic criticisms of the last two drafts.

stress the empirical generality of the power function. Were it not for the fact that some psychophysicists are uneasy about these methods, which seem to rest heavily upon our experience with the number system, the point would seem to be established. In an effort to bypass these objections, Stevens (1959) has recently had subjects match values between pairs of continua, and he finds that the resulting relations are power functions whose exponents can be predicted from the magnitude scales of the separate variables. Thus, although much remains to be learned about the "direct" methods of scaling, the resulting power functions appear to summarize an interesting body of data.

Given these empirical results, one is challenged to develop a suitable formal theory from which they can be shown to follow. There can be little doubt that, as a starting point, certain commonly made assumptions are inappropriate: equality of jnd's, equally often noticed differences, and Thurstone's equal variance assumption. Since, however, differences stand in the same—logarithmic—relation to ratios as Fechner's law does to the power function, a reasonable starting point might seem to be the assumption that the subjective ratio of stimuli one jnd apart is a constant independent of the stimulus intensity. Obvious as the procedure may seem, in my opinion it will not do. Although generations of psychologists have managed to convince themselves that the equal-jnd assumption is plausible, if not obvious, it is not and never has been particularly compelling; and in this respect, an equal-ratio assumption is not much different. This is not to deny that subjective continua may have the equal-ratio property—they must if the power law is correct and Weber's law holds—but rather to argue that such an as-

sumption is too special to be acceptable as a basic axiom in a deductive theory.

Elsewhere (Luce, in press), I have suggested another approach. An axiom, or possible law, of wide applicability in the study of choice behavior, may be taken in conjunction with the linear generalization of Weber's law to demonstrate the existence of a scale that is a power function of the physical continuum. Although that theory leads to what appears to be the correct form, it is open to two criticisms. First, the exponent predicted from discrimination data is at least an order of magnitude larger than that obtained by direct scaling methods. Second, the theory is based upon assumptions about discriminability, and these are not obviously relevant to a scale determined by another method. Scales of apparent magnitude may be related to jnd scales, but it would be unwise to take it for granted that they are.

The purpose of this paper is to outline still another approach to the problem, one that is not subject to the last criticism. The results have applicability far beyond the bounds of psychophysics, for they concern the general question of the relation between measurement and substantive theories.

#### TYPES OF SCALES

Although familiarity may by now have dulled our sense of its importance, Stevens' (1946, 1951) stress upon the transformation groups that leave certain specified scale properties invariant must, I think, be considered one of the more striking contributions to the discussion of measurement in the past few decades. Prior to his work, most writers had put extreme emphasis upon the property of "additivity," which is a characteristic of much physical measurement (Cohen & Nagel, 1934). It was held that this property is fundamental to scientific measurement and,

indeed, the term "fundamental measurement" was applied only to these scales. This contention, however, puts the nonphysical sciences in a most peculiar fix. Since no one has yet discovered an "additive" psychological variable, it would seem that psychology can have no fundamental measures of its own. This conclusion might be acceptable if we could define psychological measures in terms of the fundamental physical scales, i.e., as "derived" scales, but few of the things we want to measure seem to be definable in this way. So either rigorous psychological measurement must be considered impossible or additive empirical operations must not be considered essential to measurement. What is important is not additivity itself, but the fact that, when it is coupled with other plausible assumptions, it sharply restricts the class of transformations that may be applied to the resulting scale. Specifically, it makes the scale unique except for multiplication by positive constants, i.e., changes of unit. Additivity is not the only property that an assignment of numbers to objects or events may have which sharply limits the admissible transformations. Some of these other properties appear applicable and relevant to psychological variables, and so in this sense psychological measurement appears to be possible.

By a *theory of measurement*, I shall mean the following. One or more operations and relations are specified over a set of objects or events (the variable), and they are characterized by a number of empirically testable assumptions. In addition, it must be possible to assign numbers to the objects and to identify numerical operations and relations with the empirical operations and relations in such a way that the numerical operations represent (are isomorphic to) the empirical ones. In other words, we have a measurement

theory whenever (a) we have a system of rules for assigning numerical values to objects that are interrelated by assumptions about certain empirical operations involving them, and (b) these rules let us set up an isomorphic relation between some properties of the number system and some aspects of the empirical operations.

One of the simplest examples of a theory of measurement is a finite set (of goods) ordered by a binary (preference) relation  $P$  that is assumed to be antisymmetric and transitive. A scale  $u$  can be assigned to the set in such a manner that it represents  $P$  in the sense that  $xPy$  if and only if  $u(x) > u(y)$ .

By the *scale type*, I shall mean the group of transformations that result in other isomorphic representations of the measurement theory. In the preceding example any strictly monotonic increasing transformation will do, and scales of this type are known as ordinal. Any transformation chosen from the scale type will be said to be an *admissible transformation*.

It should be re-emphasized that quite divergent measurement theories may lead to the same scale type. For example, Case V of Thurstone's law of comparative judgment (1927) and the von Neumann-Morgenstern utility axioms (1947) both result in interval scales (of something), yet the basic terms and assumptions involved are totally different, even though both theories can be applied to the same subject matter. Of course, the resulting interval scales may not be linearly related, for they may be measuring different things.

A measurement theory may be contrasted with what I shall call a *substantive theory*. The former involves operations and assumptions only about a single class of objects which is treated as a unitary variable, whereas the lat-

ter involves relations among two or more variables. In practice, substantive theories are usually stated in terms of functional relations among the scales that result from the several measurement theories for the variables involved.

For a number of purposes, the scale type is much more crucial than the details of the measurement theory from which the scale is derived. For example, much attention has been paid to the limitations that the scale type places upon the statistics one may sensibly employ. If the interpretation of a particular statistic or statistical test is altered when admissible scale transformations are applied, then our substantive conclusions will depend upon which arbitrary representation of the scale we have used in making our calculations. Most scientists, when they understand the problem, feel that they should shun such statistics and rely only upon those that exhibit the appropriate invariances for the scale type at hand. Both the geometric and arithmetic means are legitimate in this sense for ratio scales (unit arbitrary), only the latter is legitimate for interval scales (unit and zero arbitrary), and neither for ordinal scales. For fuller discussions, see Stevens: 1946, 1951, 1955; for a somewhat less strict interpretation of the conclusions, see Mosteller, 1958.

A second place where the transformation group imposes limitations is in the construction of substantive theories. These limitations seem to have received far less attention than the statistical questions, even though they are undoubtedly more fundamental. The remainder of the paper will attempt to formulate the relation between scale types and functional laws, and to answer the question what psychophysical laws are possible. As already pointed out, these issues have scientific relevance beyond psychophysics.

## A PRINCIPLE OF THEORY CONSTRUCTION

In physics one finds at least two classes of basic assumptions: specific empirical laws, such as the universal law of gravitation or Ohm's law, and a priori principles of theory construction, such as the requirement that the laws of mechanics should be invariant under uniform translations and rotations of the coordinate system. Other laws, such as the conservation of energy, seem to have changed from the empirical to the a priori category during the development of physics. In psychology more stress has been put on the discovery of empirical laws than on the formulation of guiding principles, and the search for empirical relations tends to be pursued without the benefit of explicit statements about what is and is not an acceptable theory.<sup>2</sup> Since such principles have been used effectively in physics to limit the possible physical laws, one wonders whether something similar may not be possible in psychology.

Without such principles, practically any relation is a priori possible, and the correct one is difficult to pin down by empirical means because of the ever present errors of observation. The error problem is particularly acute in the behavioral sciences. On the other hand, if a priori consideration about what constitutes an acceptable theory limits us to some rather small set of possible laws, then fairly crude obser-

<sup>2</sup> Two attempts to introduce and use such statements in behavioral problems are the combining of classes condition in stochastic learning theory (Bush, Mosteller, & Thompson, 1954) and some work on the form of the utility function for money which is based upon the demand that certain game theory solutions should remain unchanged when a constant sum of money is added to all the payoffs (Kemeny & Thompson, 1957). In neither case do the conditions seem particularly compelling.

vations may sometimes suffice to decide which law actually obtains.

The principle to be suggested appears to be a generalization of one used in physics. It may be stated as follows.

A substantive theory relating two or more variables and the measurement theories for these variables should be such that:

1. (*Consistency of substantive and measurement theories*) Admissible transformations of one or more of the independent variables shall lead, via the substantive theory, only to admissible transformations of the dependent variables.

2. (*Invariance of the substantive theory*) Except for the numerical values of parameters that reflect the effect on the dependent variables of admissible transformations of the independent variables, the mathematical structure of the substantive theory shall be independent of admissible transformations of the independent variables.

In this principle, and in what follows, the terms independent and dependent variables are used only to distinguish the variables to which arbitrary, admissible transformations are imposed from those for which the transformations are determined by the substantive theory. As will be seen, in some cases the labeling is truly arbitrary in the sense that the substantive theory can be written so that any variable appears either in the dependent or independent role, but in other cases there is a true asymmetry in the sense that some variables must be dependent and others independent if any substantive theory relates them at all.

One can hardly question the consistency part of the principle. If an admissible transformation of an independent variable leads to an inadmissi-

ble transformation of a dependent variable, then one is simply saying that the strictures imposed by the measurement theories are incompatible with those imposed by the substantive theory. Such a logical inconsistency must, I think, be interpreted as meaning that something is amiss in the total theoretical structure.

The invariance part is more subtle and controversial. It asserts that we should be able to state the substantive laws of the field without reference to the particular scales that are used to measure the variables. For example, we want to be able to say that Ohm's law states that voltage is proportional to the product of resistance and current without specifying the units that are used to measure voltage, resistance, or current. Put another way, we do not want to have one law when one set of units is used and another when a different set of units is used. Although this seems plausible, there are examples from physics that can be viewed as a particular sort of violation of Part 2; however, let us postpone the discussion of these until some consequences of the principle as stated have been derived.

The meaning of the principle may be clarified by examples that violate it. Suppose it is claimed that two ratio scales are related by a logarithmic law. An admissible transformation of the independent variable  $x$  is multiplication by a positive constant  $k$ , i.e., a change of unit. However, the fact that  $\log kx = \log k + \log x$  means that an inadmissible transformation, namely, a change of zero, is effected on the dependent variable. Hence, the logarithm fails to meet the consistency requirement. Next, consider an exponential law, then the transformation leads to  $e^{kx} = (e^x)^k$ . This can be viewed either as a violation of consistency or of invariance. If the law is exponential, then the dependent vari-

able is raised to a power, which is inconsistent with its being a ratio scale. Alternatively, the dependent variable may be taken to be a ratio scale, but then the law is not invariant because it is an exponential raised to a power that depends upon the unit of the independent variable.

#### AN APPLICATION OF THE PRINCIPLE

Most of the physical measures entering into psychophysics are idealized in physical theories in such a way that they form either ratio or interval scales. Mass, length, pressure, and time durations are measured on ratio scales, and physical time (not time durations), ordinary temperature, and entropy are measured on interval scales. Of course, differences and derivatives of interval scale values constitute ratio scales.

Although most psychological scales in current use can at best be considered to be ordinal, those who have worked on psychological measurement theories have attempted to arrive at scales that are either ratio or interval, preferably the former. Examples: the equally often noticed difference assumption and the closely related Case V of Thurstone's "law of comparative judgment" lead to interval scales; Stevens has argued that magnitude estimation methods result in ratio scales (but no measurement theory has been offered in support of this plausible belief); and I have given sufficient conditions to derive a ratio scale from discrimination data. Our question here, however, is not how well psychologists have succeeded in perfecting scales of one type or another, but what a knowledge of scale types can tell us about the relations among scales.

In addition to these two common types of scales, there is some interest in what have been called logarithmic

interval scales (Stevens, 1957). In this case the admissible transformations are multiplications by positive constants and raising to positive powers, i.e.,  $kx^c$ , where  $k > 0$  and  $c > 0$ . The name applied to this scale type reflects the fact that  $\log x$  is an interval scale, since the transformed scale goes into  $c \log x + \log k$ . We will consider all combinations of ratio, interval, and logarithmic interval scales.

Because this topic is more general than psychophysics, I shall refer to the variables as independent and dependent rather than physical and psychological. Both variables will be assumed to form numerical continua having more than one point. Let  $x \geq 0$  denote a typical value of the independent variable and  $u(x) \geq 0$  the corresponding value of the dependent variable, where  $u$  is the unknown functional law relating them. Suppose, first, that both variables form ratio scales. If the unit of the independent variable is changed by multiplying all values by a positive constant  $k$ , then according to the principle stated above only an admissible transformation of the dependent variable, namely multiplication by a positive constant, should result and the form of the functional law should be unaffected. That is to say, the changed unit of the dependent variable may depend upon  $k$ , but it shall not depend upon  $x$ , so we denote it by  $K(k)$ . Casting this into mathematical terms, we obtain the functional equation

$$u(kx) = K(k)u(x)$$

where  $k > 0$  and  $K(k) > 0$ .

Functional equations for the other cases are arrived at in a similar manner. They are summarized in Table 1.

The question is: What do these nine functional equations, each of which embodies the principle, imply about

TABLE 1  
THE FUNCTIONAL EQUATIONS FOR THE LAWS SATISFYING THE  
PRINCIPLE OF THEORY CONSTRUCTION

Eq. No.	Scale Types		Functional Equation	Comments
	Independent Variable	Dependent Variable		
1	ratio	ratio	$u(kx) = K(k)u(x)$	$k > 0, K(k) > 0$
2	ratio	interval	$u(kx) = K(k)u(x) + C(k)$	$k > 0, K(k) > 0$
3	ratio	log interval	$u(kx) = K(k)u(x)^{C(k)}$	$k > 0, K(k) > 0, C(k) > 0$
4	interval	ratio	$u(kx+c) = K(k,c)u(x)$	$k > 0, K(k,c) > 0$
5	interval	interval	$u(kx+c) = K(k,c)u(x) + C(k,c)$	$k > 0, K(k,c) > 0$
6	interval	log interval	$u(kx+c) = K(k,c)u(x)^{C(k,c)}$	$k > 0, K(k,c) > 0, C(k,c) > 0$
7	log interval	ratio	$u(kx^c) = K(k,c)u(x)$	$k > 0, c > 0, K(k,c) > 0$
8	log interval	interval	$u(kx^c) = K(k,c)u(x) + C(k,c)$	$k > 0, c > 0, K(k,c) > 0$
9	log interval	log interval	$u(kx^c) = K(k,c)u(x)^{C(k,c)}$	$k > 0, c > 0, K(k,c) > 0, C(k,c) > 0$

the form of  $u$ ? We shall limit our consideration to theories where  $u$  is a continuous, nonconstant function of  $x$ .

**Theorem 1.** *If the independent and dependent continua are both ratio scales, then  $u(x) = \alpha x^\beta$ , where  $\beta$  is independent of the units of both variables.<sup>3</sup>*

**Proof.** Set  $x = 1$  in Equation 1, then  $u(k) = K(k)u(1)$ . Because  $u$  is nonconstant we may choose  $k$  so that  $u(k) > 0$ , and because  $K(k) > 0$ , it follows that  $u(1) > 0$ , so  $K(k) = u(k)/u(1)$ . Thus, Equation 1 becomes  $u(kx)$

$= u(k)u(x)/u(1)$ . Let  $v = \log[u/u(1)]$ , then

$$\begin{aligned} v(kx) &= \log [u(kx)/u(1)] \\ &= \log \frac{u(k)u(x)}{u(1)u(1)} \\ &= \log [u(k)/u(1)] \\ &\quad + \log [u(x)/u(1)] \\ &= v(k) + v(x) \end{aligned}$$

Since  $u$  is continuous, so is  $v$ , and it is well known that the only continuous solutions to the last functional equation are of the form

$$\begin{aligned} v(x) &= \beta \log x \\ &= \log x^\beta \end{aligned}$$

Thus,

$$\begin{aligned} u(x) &= \alpha e^{v(x)} \\ &= \alpha x^\beta \end{aligned}$$

where  $\alpha = u(1)$ .

We observe that since

$$u(kx) = \alpha k^\beta x^\beta = \alpha' x^\beta$$

$\beta$  is independent of the unit of  $x$ , and it is clearly independent of the unit of  $u$ .

**Theorem 2.** *If the independent continuum is a ratio scale and the depend-*

<sup>3</sup> In this and in the following theorems, the statement can be made more general if  $x$  is replaced by  $x + \gamma$ , where  $\gamma$  is a constant independent of  $x$  but having the same unit as  $x$ . The effect of this is to place the zero of  $u$  at some point different from the zero of  $x$ . In psychophysics the constant  $\gamma$  may be regarded as the threshold. The presence of such a constant means, of course, that a plot of  $\log u$  vs.  $\log x$  will not in general be a straight line. If, however, the independent variable is measured in terms of deviations from the threshold, the plot may become straight. Such nonlinear plots have been observed, and in at least some instances the degree of curvature seems to be correlated with the magnitude of the threshold. Further empirical work is needed to see whether this is a correct explanation of the curvature.

ent continuum an interval scale, then either  $u(x) = \alpha \log x + \beta$ , where  $\alpha$  is independent of the unit of the independent variable, or  $u(x) = \alpha x^\beta + \delta$ , where  $\beta$  is independent of the units of both variables and  $\delta$  is independent of the unit of the independent variable.

Proof. In solving Equation 2, there are two possibilities to consider.

1. If  $K(k) \equiv 1$ , then define  $v = e^u$ . Equation 2 becomes  $v(kx) = D(k)v(x)$ , where  $D(k) = e^{C(k)} > 0$  and  $v$  is continuous, positive, and nonconstant because  $u$  is. By Theorem 1,  $v(x) = \delta x^\alpha$ , where  $\alpha$  is independent of the unit of  $x$  and where  $\delta > 0$  because, by definition,  $v > 0$ . Taking logarithms,  $u(x) = \alpha \log x + \beta$ , where  $\beta = \log \delta$ .

2. If  $K(k) \neq 1$ , then let  $u$  and  $u^*$  be two different solutions to the problem, and define  $w = u^* - u$ . It follows immediately from Equation 2 that  $w$  must satisfy the functional equation  $w(kx) = K(k)w(x)$ . Since both  $u$  and  $u^*$  are continuous, so is  $w$ ; however, it may be a constant. Since  $K(k) \neq 1$ , it is clear that the only constant solution is  $w = 0$ , and this is impossible since  $u$  and  $u^*$  were chosen to be different. Thus, by Theorem 1,  $w(x) = \alpha x^\beta$ . Substituting this into the functional equation for  $w$ , it follows that  $K(k) = k^\beta$ . Then setting  $x = 0$  in Equation 2, we obtain  $C(k) = u(0) \times (1 - k^\beta)$ . We now observe that  $u(x) = \alpha x^\beta + \delta$ , where  $\delta = u(0)$ , is a solution to Equation 2:

$$\begin{aligned} u(kx) &= \alpha k^\beta x^\beta + \delta \\ &= \alpha k^\beta x^\beta + u(0)k^\beta + u(0) - u(0)k^\beta \\ &= k^\beta u(x) + u(0)(1 - k^\beta) \\ &= K(k)u(x) + C(k) \end{aligned}$$

Any other solution is of the same form because

$$\begin{aligned} u^*(x) &= u(x) + w(x) \\ &= \alpha x^\beta + \delta + \alpha x^\beta \\ &= (\alpha + a)x^\beta + \delta \end{aligned}$$

It is easy to see that  $\delta$  is independent of the unit of  $x$  and  $\beta$  is independent of both units.

A much simpler proof of this theorem can be given if we assume that  $u$  is differentiable in addition to being continuous. Since the derivative of an interval scale is a ratio scale, it follows immediately that  $du/dx$  satisfies

Equation 1 and so, by Theorem 1,  $\frac{du}{dx} = \alpha x^\beta$ . Integrating, we get

$$u(x) = \begin{cases} \frac{\alpha}{\beta+1} x^{\beta+1} + \delta & \text{if } \beta \neq -1 \\ \alpha \log x + \delta & \text{if } \beta = -1 \end{cases}$$

**Theorem 3.** *If the independent continuum is a ratio scale and the dependent continuum is a logarithmic interval scale, then either  $u(x) = \delta e^{\alpha x^\beta}$ , where  $\alpha$  is independent of the unit of the dependent variable,  $\beta$  is independent of the units of both variables and  $\delta$  is independent of the unit of the independent variable, or  $u(x) = \alpha x^\beta$ , where  $\beta$  is independent of the units of both variables.*

Proof. Take the logarithm of Equation 3 and let  $v = \log u$ :

$$v(kx) = K^*(k) + C(k)v(x)$$

where  $K^*(k) = \log K(k)$ . By Theorem 2, either

$$v(x) = \alpha x^\beta + \delta^* \text{ or } v(x) = \beta \log x + \alpha^*$$

Taking exponentials, either

$$u(x) = \delta e^{\alpha x^\beta} \text{ or } u(x) = \alpha x^\beta$$

where  $\delta = e^{\delta^*}$  and, in the second equation,  $\alpha = e^{\alpha^*}$ .

**Theorem 4.** *If the independent continuum is an interval scale, then it is impossible for the dependent continuum to be a ratio scale.*

Proof. Let  $c = 0$  in Equation 4, then by Theorem 1 we know  $u(x) = \alpha x^\beta$ .

Now set  $k = 1$  and  $c \neq 0$  in Equation 3:

$$\alpha(x + c)^\beta = K(1, c)\alpha x^\beta$$

so

$$x + c = K(1, c)^{1/\beta}x$$

which implies  $x$  is a constant, contrary to our assumption that both continua have more than one point.

**Theorem 5.** *If the independent and dependent continua are both interval scales, then  $u(x) = \alpha x + \beta$ , where  $\beta$  is independent of the unit of the independent variable.*

**Proof.** If we let  $c = 0$ , then Equation 5 reduces to Equation 2 and so Theorem 2 applies. If  $u(x) = \alpha \log x + \beta$ , then choosing  $k = 1$  and  $c \neq 0$  in Equation 5 yields

$$\alpha \log(x + c) + \beta = K(1, c)\alpha \log x + K(1, c)\beta + C(1, c)$$

By taking the derivative with respect to  $x$ , it is easy to see that  $x$  must be a constant, which is impossible.

Thus, we must conclude that  $u(x) = \alpha x^\beta + \beta$ . Again, set  $k = 1$  and  $c \neq 0$ ,

$$\alpha(x + c)^\beta = K(1, c)\alpha x^\beta + K(1, c)\beta + C(1, c)$$

If  $\beta \neq 1$ , then differentiate with respect to  $x$ :

$$\alpha\beta(x + c)^{\beta-1} = K(1, c)\alpha\beta x^{\beta-1}$$

which implies  $x$  is a constant, so we must conclude  $\beta = 1$ . It is easy to see that  $u(x) = \alpha x + \beta$  satisfies Equation 5.

**Theorem 6.** *If the independent continuum is an interval scale and the dependent continuum is a logarithmic interval scale, then  $u(x) = \alpha e^{\beta x}$ , where  $\alpha$  is independent of the unit of the independent variable and  $\beta$  is independent of the unit of the dependent variable.*

**Proof.** Take the logarithm of Equation 6 and let  $v = \log u$ :

$$v(kx + c) = K^*(k, c) + C(k, c)v(x)$$

where  $K^*(k, c) = \log K(k, c)$ . By Theorem 5,

$$v(x) = \beta x + \alpha^*$$

so

$$u(x) = \alpha e^{\beta x}$$

where  $\alpha = e^{\alpha^*}$ .

**Theorem 7.** *If the independent continuum is a logarithmic interval scale, then it is impossible for the dependent continuum to be a ratio scale.*

**Proof.** Let  $v(\log x) = u(x)$ , i.e.,  $v(y) = u(e^y)$ , then Equation 7 becomes

$$v(\log k + c \log x) = K(k, c)u(\log x)$$

Thus,  $\log x$  is an interval scale and  $v$  is a ratio scale, which by Theorem 4 is impossible.

**Theorem 8.** *If the independent continuum is a logarithmic interval scale and the dependent continuum is an interval scale, then  $u(x) = \alpha \log x + \beta$ , where  $\alpha$  is independent of the unit of the independent variable.*

**Proof.** Let  $v(\log x) = u(x)$ , then Equation 8 becomes

$$v(\log k + c \log x) = K(k, c)v(\log x) + C(k, c)$$

so  $\log x$  and  $v$  are both interval scales. By Theorem 5,

$$u(x) = v(\log x) = \alpha \log x + \beta$$

**Theorem 9.** *If the independent and dependent continua are both logarithmic interval scales, then  $u(x) = \alpha x^\beta$ , where  $\beta$  is independent of the units of both the independent and dependent variables.*

Proof. Take the logarithm of Equation 9 and let  $v = \log u$ :

$$v(kx^c) = K^*(k, c) + C(k, c)v(x)$$

where  $K^*(k, c) = \log K(k, c)$ . By Theorem 8,

$$v(x) = \beta \log x + \alpha^*$$

so

$$\begin{aligned} u(x) &= e^{v(x)} \\ &= \alpha x^\beta \end{aligned}$$

where  $\alpha = e^{\alpha^*}$ .

#### ILLUSTRATIONS

It may be useful, prior to discussing these results, to cite a few familiar laws that accord with some of them. The best source of examples is classical physics, where most of the fundamental variables are idealized as continua that form either ratio or interval scales. No attempt will be made to illustrate the results concerning logarithmic interval scales, because no actual use of scales of this type seems to have been made.

The variables entering into Coulomb's law, Ohm's law, and Newton's gravitation law are all ratio scales, and in each case the form of the law is a power function, as called for by Theorem 1. Additional examples of Theorem 1 can be found in geometry since length, area, and volume are ratio scales; thus the dependency of the volume of a sphere upon its radius or of the area of a square on its side are illustrations.

Other important variables such as energy and entropy form interval scales, and we can therefore anticipate that as dependent variables they will illustrate Theorem 2. If a body of constant mass is moving at velocity  $v$ , then its energy is of the form  $\alpha v^2 + \delta$ . If the temperature of a perfect gas is constant, then as a function of pressure  $p$  the entropy of the gas is of the

form  $\alpha \log p + \beta$ . No examples, of course, are possible for Theorem 4.

As an example of Theorem 5 we may consider ordinary temperature, which is frequently measured in terms of the length of a column of mercury. Although length as a measure forms a ratio scale, the length of a column of mercury used to measure temperature is an interval scale (subject to the added constraint that the length is positive), since we may choose any initial length to correspond to a given temperature, such as the freezing point of water. If the temperature scale is also an interval scale, as is usually assumed, then the only relation possible according to Theorem 5 is the linear one.

#### DISCUSSION

Some with whom I have discussed these theorems—which from a mathematical point of view are not new—have had strong misgivings about their interpretation; the feeling is that something of a substantive nature must have been smuggled into the formulation of the problem. They argue that practically any functional relation can hold between two variables and that it is an empirical, not a theoretical, matter to ascertain what the function may be in specific cases. To support this view and to challenge the theorems, they have cited examples from physics, such as the exponential law of radioactive decay or some sinusoidal function of time, which seem to violate the theorems stated above. We must, therefore, examine the ways in which these examples bypass the rather strong conclusions of the present theory.

All physical examples which have been suggested to me as counterexamples to the theorems have a common form: the independent variable is a ratio scale, but it enters into

the equation in a dimensionless fashion. For example, some identifiable value of the variable is taken as the reference level  $x_0$ , and all other values are expressed in reference to it as  $x/x_0$ . The effect of this is to make the quantity  $x/x_0$  independent of the unit used to measure the variable, since  $kx/kx_0 = x/x_0$ . In periodic functions of time, the period is often used as a reference level. Slightly more generally, the independent variable only appears multiplied by a constant  $c$  whose units are the inverse of those of  $x$ . Thus, whenever the unit of  $x$  is changed by multiplying all values by a constant  $k > 0$ , it is necessary to adjust the unit of  $c$  by multiplying it by  $1/k$ . But this means that the product is independent of  $k$ :  $(c/k)(kx) = cx$ . The time constant in the law of radioactive decay is of this nature.

There are two ways to view these examples in relation to the principle stated above. If the ratio scale  $x$  is taken to be the independent variable, then the invariance part of the principle is not satisfied by these laws. If, however, for the purpose of the law under consideration the dimensionless quantity  $cx$  is treated as the variable, then no violation has occurred. Although surprising at first glance, it is easy to see that the principle imposes no limitations upon the form of the law when the independent variable is dimensionless, i.e., when no transformations save the identity are admissible.

We are thus led to the following conclusion. Either the independent variable is a ratio scale that is multiplied by a dimensional constant that makes the product independent of the unit of the scale, in which case there is no restriction upon the laws into which it may enter, or the independent variable is not rendered dimensionless, in which case the laws must be of the

form described by the above theorems. Both situations are found in classical physics, and one wonders if there is any fundamental difference between them. I have not seen any discussion of the matter, and I have only the most uncertain impression that there is a difference. In many physical situations where a dimensional constant multiplies the independent variable, the dependent variable is bounded. This is true of both the decay and periodic laws. Usually, the constant is expressed in some natural way in terms of the bounds, as, for example, the period of a periodic function. Whether dimensional constants can legitimately be used in other situations, or whether they can always be eliminated, is not at all apparent to me.

One may legitimately question which of these alternatives is applicable to psychophysics, and the answer is far from clear. The widespread use of, say, the threshold as a reference level seems at first to suggest that psychophysical laws are to be expressed in terms of dimensionless quantities; however, the fact that this is done mainly to present results in decibels may mean no more than that the given ratio scale is being transformed into an interval scale in accordance with Theorem 2:

$$\begin{aligned} y &= \alpha \log x/x_0 \\ &= \alpha \log x + \beta \end{aligned}$$

where

$$\beta = -\alpha \log x_0$$

In addition to dimensionless variables as a means of by-passing the restrictions imposed by scale types, three other possibilities deserve discussion.

First, the idealization that the scales form mathematical continua and that they are related by a continuous func-

tion may not reflect the actual state of affairs in the empirical world. It is certainly true that, in detail, physical continua are not mathematical continua, and there is ample reason to suspect that the same holds for psychological variables. But the assumptions that stimuli and responses both form continua are idealizations that are difficult to give up; to do so would mean casting out much of psychophysical theory. Alternatively, we could drop the demand that the function relating them be continuous, but it is doubtful if this would be of much help by itself. The discontinuous solutions to, say, Equation 1 are manifold and extremely wild in their behavior. They are so wild that it is difficult to say anything precise about them at all (see Hamel, 1905; Jones: 1942a, 1942b), and it is doubtful that such solutions represent empirical laws.

Second, casual observation suggests that it might be appropriate to assume that at least the dependent variable is bounded, e.g., that there is a psychologically maximum loudness. Although plausible, boundedness cannot be imposed by itself since, as is shown in the theorems, all the continuous solutions to the appropriate functional equations are unbounded if the functions are increasing, as they must be for empirical reasons. It seems clear that boundedness of the dependent variable is intimately tied up either with introducing a reference level so that the independent variable is an absolute scale or with some discontinuity in the formulation of the problem, possibly in the nature of the variables or possibly in the function relating them. Actually, one can establish that it must be in the nature of the variables. Suppose, on the contrary, that the variables are ratio scales that form numerical continua

and that they are related by a function  $u$  that is nonnegative, nonconstant, and monotonic increasing, but not necessarily continuous. We now need only show that  $u$  cannot be bounded to show that the discontinuity must exist in the variable. Suppose, therefore, that it is bounded and that the bound is  $M$ . By Equation 1,  $u(kx) = K(k)u(x) \leq M$ , so  $u(x) \leq M/K(k)$ . For  $k \geq 1$ , the monotonicity of  $u$  implies that  $u(x) \leq u(kx) = K(k)u(x)$ , so choosing  $u(x) > 0$  we see that  $K(k) \geq 1$ . If for some  $k \geq 1$ ,  $K(k) > 1$ , then  $K$  can be made arbitrarily large since, for any integer  $n$ ,  $K(k^n) = K(k)^n$ , but since  $u(x) \leq \frac{M}{K(k)}$ , this implies  $u \equiv 0$ , contrary to assumption. Thus, for all  $k \geq 1$ ,  $K(k) = 1$ , which by Equation 1, means  $u(kx) = u(x)$ , for all  $x$  and  $k \geq 1$ . This in turn implies  $u$  is a constant, which again is contrary to assumption. Thus, we have established our claim that some discontinuity must reside in the nature of the variables.

Third, in many situations, there are two or more independent variables; for example, both intensity and frequency determine loudness. Usually we hold all but one variable constant in our empirical investigations, but the fact remains that the others are there and that their presence may make some difference in the total range of possible laws. For example, suppose there are two independent variables,  $x$  and  $y$ , both of which form ratio scales and that the dependent variable  $u$  is also a ratio scale, then the analogue of Equation 1 is

$$u(kx, hy) = K(k, h)u(x, y)$$

where  $k > 0$ ,  $h > 0$ , and  $K(k, h) > 0$ . We know by Theorem 1 that if we hold one variable, say  $y$ , fixed at some

value and let  $h = 1$ , then the solution must be of the form

$$u(x, y) = \alpha(y)x^{\beta(y)}$$

But holding  $x$  constant and letting  $h = 1$ , we also know that it must be of the form

$$u(x, y) = \delta(x)y^{\epsilon(x)}$$

Thus,

$$\alpha(y)x^{\beta(y)} = \delta(x)y^{\epsilon(x)}$$

If we restrict ourselves to  $u$ 's having partial derivatives of both variables, this equation can be shown (see Section 2.C.2 of Luce [in press]) to have solutions only of the form:

$$u(x, y) = ax^by^{c+d \log x}$$

Thus, the principle again severely restricts the possible laws, even when we admit more than one independent variable.<sup>4</sup>

It must be emphasized that the remark in Footnote 3 does not apply here. If a function that depends upon one independent variable is added to the other, e.g.,

$$u(x, y) = \alpha(y)[x + \gamma(y)]^{\beta(y)}$$

then wholly new solution possibilities exist (see Section 2.C.3 of Luce [in press]).

In sum, there appear to be two ways around the restrictions set forth in the theorems. The first can be viewed either as a rejection of Part 2 of the principle or as the creation of a dimensionless independent variable from a ratio scale; it involves the presence of dimensional constants that cancel out

the dimensions of the independent variables. This appears to be particularly appropriate if the dependent variable has a true, well-defined bound. The second is to reject the idealization of the variables as numerical continua and, possibly, to assume that they are bounded.

On the other hand, if the theorems are applicable, then the possible psychophysical (and other) laws become severely limited. Indeed, they are so limited that one can argue that the important question is not to determine the forms of the laws, but rather to create empirically testable measurement theories for the several psychophysical methods in order that we may know for certain what types of scales are being obtained. Once this is known, the form of the psychophysical functions is determined except for some numerical constants. In the meantime, however, experimental determinations of the form of the psychophysical functions by methods for which no measurement theories exist provides at least indirect evidence of the type of scale being obtained. For example, the magnitude methods seem to result in power functions, which suggests that the psychological measure is either a ratio or logarithmic interval scale, not an interval scale. Since the results from cross-modality matchings tend to eliminate the logarithmic interval scale as a possibility, there is presumptive evidence that these methods yield ratio scales, as Stevens has claimed.

#### SUMMARY

The following problem was considered. What are the possible forms of a substantive theory that relates a dependent variable in a continuous manner to an independent variable? Each variable is idealized as a nu-

<sup>4</sup> The use of this argument to arrive at the form of  $u(x, y)$  seems much more satisfactory and convincing than the heuristic development given in Section 2.C of Luce (in press), and the empirical suggestions given there should gain correspondingly in interest as a result of the present work.

TABLE 2  
THE POSSIBLE LAWS SATISFYING THE PRINCIPLE OF THEORY CONSTRUCTION

Scale Types		Possible Laws	Comments <sup>a</sup>
Independent Variable	Dependent Variable		
ratio	ratio	$u(x) = \alpha x^\beta$	$\beta/x; \beta/u$
ratio	interval	$u(x) = \alpha \log x + \beta$	$\alpha/x$
		$u(x) = \alpha x^\beta + \delta$	$\beta/x; \beta/u; \delta/x$
ratio	log interval	$u(x) = \delta e^{\alpha x^\beta}$	$\alpha/u; \beta/x; \beta/u; \delta/x$
		$u(x) = \alpha x^\beta$	$\beta/x; \beta/u$
interval	ratio	impossible	
interval	interval	$u(x) = \alpha x + \beta$	$\beta/x$
interval	log interval	$u(x) = \alpha e^{\beta x}$	$\alpha/x; \beta/u$
log interval	ratio	impossible	
log interval	interval	$u(x) = \alpha \log x + \beta$	$\alpha/x$
log interval	log interval	$u(x) = \alpha x^\beta$	$\beta/x; \beta/u$

<sup>a</sup> The notation  $\alpha/x$  means " $\alpha$  is independent of the unit of  $x$ ."

merical continuum and is restricted by its measurement theory to being either a ratio, an interval, or a logarithmic interval scale. As a principle of theory construction, it is suggested that transformations of the independent variable that are admissible under its measurement theory shall not result in inadmissible transformations of the dependent variable (consistency) and that the form of the functional relation between the two variables shall not be altered by admissible transformation of the independent variable (invariance). This principle limits significantly the possible laws relating the two continua, as shown in Table 2.

These results do not hold in two important circumstances. First, if the independent variable is a ratio scale that is rendered dimensionless by multiplying it by a constant having units reciprocal to those of the independent variable, then either the principle has no content or it is violated, depending upon how one wishes to look at the matter. Second, if the variables are discrete rather than continuous, or if the functional relation is discontinuous, then laws other than those given in Table 2 are possible.

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## STIMULUS CHANGE, REACTIONS TO NOVELTY, AND RESPONSE DECREMENT<sup>1</sup>

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A response can occur only within a certain range of variation of the stimulus situation in which that response was acquired or practiced. Within this range, any alterations in the situation tend only to modify the trained response. The typical modification is decrease in response probability or, simply, response decrement, as described by increased latency, lowered frequency of occurrence, lowered resistance to extinction, and by other commonly employed indices of response strength. Thus, when certain stimulus elements of a training situation,  $S$ , are altered to make it  $S'$ , the strength of the trained response in the test situation ( $S'$ ) is reduced as measured by one or the other of the indices. This paper suggests how reactions to the "novelty" provided by the test situation may determine such response decrement and, by specifying the nature and determinants of "novelty reactions," develops a basis for predicting the effects on a trained response of a given stimulus change.

### THE COMMON-ELEMENTS MODEL

Perhaps the easiest way to state the present problem is to link it to Guthrie's (1935) common-elements model. According to this view, any learning situation consists of a large number of stimulus elements, and in the course

of the training of a response a progressively larger number of these elements becomes conditioned to the response. Thus, at any given stage of learning, some of the stimulus elements are conditioned to the response, and the remaining elements are not conditioned to that response. Estes (1950) and Bush and Mosteller (1951) have extended this model, postulating that the probability of occurrence of the trained response on the next following trial is a function of the proportion of the total number (or some measure) of stimulus elements in the situation that are conditioned. With the aid of some simplifying assumptions, they are able to state that any change in the situation will introduce new stimulus elements into it, thus reducing the proportion of conditioned elements and decreasing response strength, provided the altered situation does not contain any new stimulus elements conditioned to the same response. From this formulation it follows that the amount of response decrement will be inversely proportional to the number of conditioned stimulus elements shared by the training and test situations, assuming the total number of stimulus elements in the two situations to be equal. A rough verification of this prediction has been obtained in an experiment by Fink and Patton (1953). They found the decrement in a drinking response to be directly proportional to the number of stimulus characteristics (visual, tactual, and auditory) changed from the training to the test situation.

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The obvious next problem concerns the factors underlying such response decrements. Why does a decrease in the number of conditioned stimulus elements shared by the training and test situations lead to response decrement? Estes (1950) and Bush and Mosteller (1951) answer this question simply by saying that, as the number of shared conditioned stimulus elements decreases, the tendency to make responses other than the trained response increases. These "other" responses are presumably evoked by the nonconditioned stimulus elements (nonconditioned to the trained response), which are assumed to be conditioned to a variety of responses in the animal's repertoire. However, neither Estes nor Bush and Mosteller state anything about the nature of such interfering responses.

Restle's (1955) formulation of the learning process takes us a step further. According to him, in learning to give a particular response in a particular stimulus situation two separate things happen: the animal learns to respond to the relevant cues, those that are consistently related to reinforcement, and, at the same time, learns not to respond to the irrelevant cues in the experimental situation. Relevant cues become conditioned to the required response, and the irrelevant cues become "adapted to" and cease to evoke the interfering responses that they evoked before. Thus, according to Restle, the extent of adaptation to the irrelevant cues contributes to the occurrence of the required response just as does the extent of conditioning to the relevant cues. The unadapted responses interfere with (i.e., reduce response strength of) the trained response. It follows that, as soon as any aspect of the training situation is altered, some new unadapted responses will begin to interfere with the trained response, and

such responses will continue to occur until the stimulus elements that evoke them have been adapted to. However, Restle does not specify the nature of such unadapted responses or their relation to the altered stimulus elements. In the next section it is argued that by specifying the nature and determinants of the unadapted, interfering, responses it is possible to achieve a more adequate basis for predicting the type and degree of response decrement that would result from a given stimulus change.

#### INTERFERING RESPONSES AS REACTIONS TO NOVELTY

The central idea of this paper is that the interfering responses responsible for the stimulus-change induced response decrement are mainly the unadapted reactions evoked by the novelty provided by the altered stimulus elements. And, because they evoke such "novelty reactions," the new stimulus elements modify the trained response in a particular, specifiable way, a way that is different from what would be predicted if the only contribution of the new elements was that they were nonconditioned. A consideration of the nature of reactions to novelty is thus thought to be essential for understanding the type and degree of response decrements brought about by stimulus change.

The concept of *novelty* or *stimulus change* has meaning only with reference to the past experience of animals. Stimuli are novel or strange for an animal only because it has not been exposed to them before (Dember & Earl, 1957; Hebb, 1946). Nothing is novel in and of itself. Thus, the introduction of new stimulus elements provides novelty for an animal only after it has been exposed to (familiarized with) an experimental situation of which those stimulus elements are not

normally a part. The degree of novelty provided by a change from the training to a testing situation is, therefore, a function not only of the number of stimulus elements in the training situation that are replaced by new elements, but also of the extent to which, in the animal's experience, the replaced stimulus elements had been a part of the training situation, and of the extent of the animal's familiarity with the new stimulus elements. However, with prior exposure to the training situation and to the new (to be introduced) stimulus elements constant for a number of groups of animals, the novelty provided by the test situation would be determined directly by the number of stimulus elements altered, assuming, of course, that the total number of stimulus elements in the two situations is equal and that it remains constant when some of the  $S$  elements are altered to make  $S'$ .

Reactions to novelty have been extensively studied in the rat (see Dember & Earl, 1957). Typically, the aspect of rat behavior that has been considered as an index of response to novelty is the amount of locomotion ("exploration") in a specified test situation. However, a recent study by Bindra and Spinner (in press) shows that novel situations evoke responses other than locomotion, and that the exact nature of these responses depends upon the degree of novelty. They housed rats in cages that possessed certain visual, auditory, and tactual characteristics. In three other (test) cages, these characteristics were altered in such a way that each test cage provided a different amount of stimulus change (or novelty) from the living cages. They found that sniffing (including sniffing plus locomotion) was evoked more frequently in the least novel test cage than in the most novel one, but the incidence of grooming

responses was the highest in the most novel test situation and the lowest in the least novel situation. They also observed progressive increases or decreases throughout the observation period in the incidence of sniffing, grooming, freezing, and locomotion. These progressive changes can be looked upon as representing habituation of the animals to the novel aspects of the test cage, for repeated exposure to a novel situation typically brings the incidence of various activities back to the normal, living cage, levels. Activities such as sniffing, grooming, and freezing cannot be considered as merely "null" activities, performed when some more meaningful activity cannot or does not occur, for in the above study they were shown to be systematically related to degree of novelty. Thus, the current studies of "exploratory" locomotion do not cover the wide range of responses that animals in fact show in situations of varying novelty. However, pending further study, it seems reasonable to suggest here that in the rat the change from the training to the test situation increases the incidence of activities such as sniffing, grooming, and freezing, and it is these activities which interfere with and reduce the response strength of a trained response. More generally, it is proposed that *any change in stimulus elements from the training to the test situation produces decrement in a trained response because, and to the extent that, the novelty provided by the altered stimulus elements evokes interfering "novelty reactions."*

The above proposition can be used to interpret the results of various experiments on response decrement brought about by stimulus change. Consider, for example, the Fink and Patton (1953) study mentioned above. That experiment does not, of course, show that the drinking response of

rats was weakened because of interference from reactions to the novelty aspect of the test conditions. However, that this may be the case is suggested by other experiments. Hall (1955) and Hurwitz and Cutts (1957) have shown in the rat that the extinction of a response is more rapid (i.e., the response is weaker) when the extinction trials are given under altered stimulus conditions than when the training and extinction situations are the same. In the first of these studies the stimulus change involved removing a part of the training apparatus, and, in the second, it involved the addition of an auditory signal. Since the addition of new stimuli, as well as the removal of familiar ones, is known to evoke novelty reactions in the rat (Berlyne, 1955; Chance & Mead, 1955; Dember & Earl, 1957), it is likely that it was the evocation of such reactions that interfered with the occurrence of the trained responses and thus hastened their extinction. Also, in many learning experiments it is incidentally observed that an unplanned change in the over-all experimental situation "distracts" the animal and produces response decrement. Thus, it seems reasonable to conclude that the declining response strength functions reported by Fink and Patton were also caused by the interfering novelty reactions evoked by the altered stimulus elements of the test situation.

However, it must be admitted that, in the absence of exact knowledge about the amount of novelty offered by a particular stimulus change, as well as about the typical reactions of a given species to that amount of novelty, it is not possible to propose any general relation between the nature and extent of stimulus change and the degree of response decrement. In the rat, what evidence is available seems to suggest that with minor changes in the experi-

mental situation, such as altering only the size of a training circle in a stimulus generalization experiment, the reactions to the change are likely to be of the sniffing, rearing, or head-turning variety, and are likely to be quickly adapted out. With more extreme types of stimulus change, such as changing the tactual and visual characteristics of a large part of the experimental situation or introducing a loud noise, the rat's reactions are likely to involve grooming and freezing, are not likely to be easily eliminated, and are thus likely to produce a marked and prolonged decrement in the trained response.

The above remarks suggest that detailed investigations of novelty reactions in experiments dealing with the effects of stimulus change may turn out to be quite fruitful. The present formulation also points to the possibility that a trained response itself may interfere with the occurrence of novelty reactions (Bindra, 1959, Ch. 7), and thus may reduce their incidence below the level at which they normally would occur in a situation of a given degree of novelty. Another feature of the proposed formulation is that a variety of phenomena involving varying degrees of stimulus change, from the stimulus generalization to the distraction-stress experiments, can be approached within a single framework. Let us now consider some specific implications of the present formulation.

#### SOME SPECIFIC IMPLICATIONS

The above line of argument concerning the factors underlying the effect of stimulus change on a trained response leads to a number of empirically testable implications. Three of these are developed here.

*Amount of training and the effects of stimulus change.* It was noted earlier that the degree of novelty offered

to an animal by a particular stimulus change is determined by the extent of the animal's prior experience with the situation in which the stimulus change is introduced. This relation between novelty and prior experience implies that, for any given stimulus change, the greater the training an animal has received in the training situation, the greater would be the novelty offered when some elements of it are altered to make it the test situation, and hence the greater would be the interfering novelty reactions evoked during the test. Assuming that the reactions to novelty evoked by the training situation reach a stable level after a few training trials, it follows from the above considerations that the greater the training given before introducing a stimulus change, the *greater* would be the relative response decrement brought about by the given stimulus change. (Relative response decrement = training performance - test performance/training performance.)

Under similar experimental conditions, the formulations of Hull and of Estes and Bush and Mosteller would lead to different predictions. First, according to Hull, in the early stages of training "stimulus generalization" is complete or nearly so, but, with increasing amounts of training, "a downward sloping gradient of primary stimulus generalization of reaction strength gradually and spontaneously develops" (Hull, 1947, p. 133). Hull (1943, p. 199) considers generalized response strength also to be a function of response strength generated during training, so that any training after a gradient has been established would result in progressively greater "generalized" response strength at each point on the gradient. These propositions would lead one to expect little or no response decrement if some stimulus change were made after only a small amount

of training, and a marked decrement if the same stimulus change were made after somewhat more (medium) training. Furthermore, if the stimulus change were made after an even larger amount of training, there would be a further increase in the absolute level of response strength throughout the gradient. However, in the latter two cases (medium and large training), for any given absolute amount of response decrement from the training to the test situation, there should be a *decrease* in relative response decrement with increased training, for the absolute level of response strength of the trained response will be higher with increased training. (Owing to the multiplicity of possible alternative sets of assumptions, other predictions can no doubt be derived from Hull's theory, but the above prediction follows most directly and obviously from the usual statement of the theory.)

Second, according to the formulations of Estes (1950; Estes & Burke, 1953) and Bush and Mosteller (1951), the probability of occurrence of a trained response in the test situation is a function, roughly, of the number of stimulus elements in the training situation that are also present in the test situation. Inasmuch as the stimulus elements that become conditioned to the response with increased training are assumed to be randomly distributed among the total stimulus elements in the situation, the proportion of the conditioned stimulus elements remaining after the stimulus change would be the same at all levels of training. Therefore, one should expect that a given stimulus change would cause the *same* relative response decrement irrespective of the amount of training given prior to the stimulus change.

Bindra and Seely (in press) have recently made an experimental test of the above three opposing predictions of

the effects of training on stimulus-change induced response decrement. They introduced a stimulus change into a runway after groups of rats had received 15, 40, or 80 training trials. They found the relative response decrement brought about by the stimulus change to increase as a function of the number of training trials. It was also found that the greater the training prior to introducing the stimulus change, the more trials were required to overcome the response decrement (i.e., to return to the prechange level of performance). These results support the novelty-reactions hypothesis and are inconsistent with the Hullian, as well as the Estes and Bush and Mosteller, type of formulation.

*Effects of prior exposure to the test situation.* If the decrease in response strength from the training to the test situation is attributed to interfering novelty reactions, exposing the animal to the test situation before any training is given should attenuate response decrement when, after training, the animal is placed in the test situation. Such an attenuation of response decrement would not be predicted from Hullian theory. According to Hull (1943), merely exposing an animal to test stimuli, without any specific training involving reinforcement (primary or secondary), should not alter the subsequent "generalized habit strength" of the response to test stimuli. The Bush and Mosteller (1951) model, inasmuch as it also makes reinforcement a necessary condition of learning, would likewise predict no effect on response decrement of pretraining unreinforced exposure to test stimuli. Since, under the stated conditions, the response to be trained would not occur consistently during the pretraining exposure to test stimuli, the contiguity-learning formulation of Estes (1950) would probably predict the same result as Hull and

Bush and Mosteller. Clearly, the prediction of attenuated response decrement that follows from the novelty-reactions hypothesis is opposed to the deduction from these other formulations.

*Different decrements in different measures of response strength.* According to the present formulation, the type and degree of response decrement depend upon the nature and extent of the interfering reactions evoked by the novelty aspect of the test situation. Now, it is likely that the interfering reactions will not occur evenly over the whole of the test situation; the parts of the test situation that represent a change from the training situation are more likely to evoke novelty reactions than are other parts of the test situation. This means that the stimulus change may have different effects on the different components of the trained response. For example, consider a simple runway situation with a starting box, an alley, and a goal box. If, after training, the stimulus change is introduced only in the starting box, it is likely that the latency of the trained running response will increase without any changes in the time taken to traverse the alley or the amount of consummatory activity in the goal box. But, if, alternatively, the stimulus change involves altering some feature of the alley, only the running time measure of response strength is likely to show decrement; similarly, if some feature of the goal box only is altered, the amount of consummatory activity may change without any corresponding changes in any of the other measures of response strength. If during the test trials, with altered stimulus elements, no reinforcement is given, a resistance to extinction measure of response strength can be obtained in all the above cases. Whether there is a response decrement as judged by this

measure would seem to depend upon the exact criterion (e.g., latency vs. running time) of extinction employed. That response decrement is in fact a function of the measure of response strength employed is shown by the difference in the degree of response decrement obtained with different measures of response strength under identical conditions of stimulus change (Heyman, 1957).

Predictions of the type stated in the last paragraph cannot be made on the basis of either the Hullian or the common-elements types of formulations as they are at present stated. Hull (1943) considers the various measures of response strength as equivalent, and Estes (1950) and Bush and Mosteller (1951), in dealing only with response frequency, have ignored the possible different outcomes with different measures of response strength.<sup>2</sup> That the various measures do not yield identical estimates of response strength has become increasingly clear in recent years (e.g., Hall & Koberick, 1952; Koberick, 1956). In order to deal with the disparate effects of stimulus change on different measures of response strength, further explications of the Hullian and common-elements formulations are needed.

#### SUMMARY AND CONCLUSIONS

If an animal is trained to give a response in a particular situation, any alteration in the stimulus characteristics of the situation typically results in a decrement in the strength of the trained response. It is generally assumed that the altered stimulus elements of the test situation evoke some "other" responses which interfere with the trained response, thus decreasing

its response strength. The aim of this paper has been to specify the nature and determinants of such interfering responses in order to develop a more adequate basis for predicting the type and degree of response decrement brought about by a given stimulus change.

Starting with Estes' and Bush and Mosteller's mathematical formulation of Guthrie's common-element model, this paper proposes an additional proposition. It states that any change in stimulus elements from the training to the test situation produces response decrement because, and to the extent that, the novelty provided by the altered stimulus elements evokes interfering reactions-to-novelty. The determinants of the degree of novelty offered by a stimulus change are discussed, and evidence is cited to show that the nature and extent of "novelty reactions" depend upon degree of novelty. Apart from pointing to the need for detailed studies of novelty reactions in experiments involving any type of stimulus change, the proposed formulation has some unique empirically testable implications. Three of these implications have been worked out in some detail.

The proposed novelty-reactions interpretation is not inherently inconsistent with the common-elements interpretation. However, the particular mathematical models developed from the latter, as seen in the works of Estes and Bush and Mosteller, seem to be concerned only with the role of conditioned stimulus elements in the occurrence of a response. Restle's formulation, inasmuch as it ascribes at least some role to (unadapted) irrelevant cues, appears to be somewhat more suitable for incorporating the novelty-reactions proposition. But considerable further elaboration of his formulation is needed before the concepts of novelty and novelty reactions

<sup>2</sup> In a recent paper in the *Nebraska Symposium on Motivation* (1958), Estes has considered this problem in some detail.

can be successfully introduced into his equations.

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## THE ROLE OF AFFECTIVE PROCESSES IN LEARNING AND MOTIVATION<sup>1</sup>

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The common sense hedonistic explanation of learning implies that subjective feelings of pleasantness and unpleasantness influence behavior. This view involves the mind-body tangle. Thorndike tried to give a better formulation with his law of effect and concepts of satisfiers and annoyers; he later revised the theory, attempting to make it more objective. Troland introduced the concepts of bene- and noci-ception, but his views have never been widely accepted.

The aim of the present paper is to show that affective processes can be studied within a strictly objective frame of reference. I will argue that the affective processes are intervening variables, in Tolman's sense. For the present they may be viewed as logical constructs which bring together in an orderly way a large body of facts. Eventually affective processes will be described in physiological terms; current research indicates that their bodily nature and locus will some day be known. In the meantime the affective processes can be anchored firmly to events within the physical world.

### THE CONSTRUCT OF AFFECTIVE PROCESSES

Let us begin by postulating that affective processes have an objective existence within the organism and that

their nature and functions can be discovered.

#### *Definition of the Affective Processes by Their Attributes*

Affective processes can be defined objectively in terms of their attributes: sign, intensity, duration:

1. *Sign.* What one observes in laboratory situations is that naive animals develop approach-maintaining or avoidance-terminating patterns of behavior. If they develop the approach-maintaining pattern, I would assume that the underlying affective process is positive in sign. If they develop the avoidance-terminating pattern, I would assume that the affective process is negative in sign. If neither positive nor negative behavior develops, I would make no assumption concerning the sign of affective arousal.

It is important to note that the bare existence of adient or abient behavior is not a sufficient ground for inferring affective processes. Approach-maintaining and avoidance-terminating behavior may be habitual, automatic and affectively indifferent; but the *development* of approach-maintaining or avoidance-terminating patterns by naive animals is the criterion for the sign of affective processes.

2. *Intensity.* In addition to sign, affective processes differ in intensity, or degree. Affective processes vary along a bipolar continuum between the extremes of maximal negative and maximal positive intensity.

One way to demonstrate the relative intensity of affective processes is to

<sup>1</sup> This paper was originally prepared for a symposium upon "Contemporary Accounts of Reinforcement," Midwestern Psychological Association, Detroit, May 2, 1958. I am indebted to my colleague, G. Robert Grice, for a critical reading of the original paper.

give animals a brief-exposure preference test with foods. A brief-exposure test is recommended because with prolonged exposures the level of acceptability of test-foods declines as the terminal state of satiation is approached.

In the brief-exposure test the animal is offered a series of choices between two test-foods (A and B). The series of choices reveals whether a preference for one food (A) or the other (B) develops. There is no way to force an animal to show a preference. Either a preference develops, with repeated choices, or it does not develop. Weak preferences, strong preferences, alternating preferences, and no preferences at all, have been found. In some tests the preference is obvious but, in others, statistical methods are needed to determine whether or not a particular body of data indicates a significant preference or a mutation of preference.

If both test-foods are accepted, I would assume that the preferred food arouses a higher intensity of positive affectivity than the nonpreferred. This is what is meant, objectively, by the statement that the preferred food is the more palatable. Again, it must be emphasized that the *development* of a preference in *naïve animals* indicates relative hedonic intensity and not the bare existence of a preference, since a preferential discrimination can be purely habitual and automatic.

3. *Duration.* In addition to sign and intensity, affective processes differ in duration and temporal course. Insofar as affective processes are induced by taste solutions, the duration of stimulation can be used to control the duration of affective arousal. The number of seconds that an animal is in contact with a food can be controlled or the number of individual licks of a fluid can be counted by an electronic device. The frequency and schedule of affective processes can also be controlled.

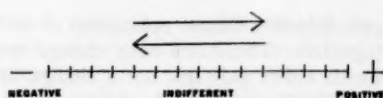


FIG. 1. The hedonic continuum.

With painful stimulations it would seem that the intensity, frequency, and schedule are all subject to precise experimental control. In addition to direct stimulation, negative affectivity can be produced by frustration and conflict; but these conditions can be controlled less precisely than the conditions of sensory stimulation.

### *The Hedonic Continuum*

The sign, intensity, and temporal changes of affective processes can be represented upon the hedonic continuum. Figure 1 shows this continuum extending from the extreme of negative affectivity (distress) to the extreme of positive affectivity (delight). Different intensities of affective arousal are represented by arbitrary units marked off upon the continuum. Midway between negative and positive affectivity is the range of indifferent, neutral processes and others that are weakly affective.

The arrows represent two opposed directions of hedonic change. The upper arrow, pointing away from the negative end and towards the positive end of the continuum, represents a kind of hedonic change that is of great importance in the organization of behavior. According to the hedonic hypothesis, neurobehavioral patterns are organized that minimize negative affectivity (distress) and maximize positive affectivity (delight). That is to say, organization is dependent upon hedonic change in the positive direction. Changes in the negative direction necessarily and frequently occur, and the lower arrow represents such changes. The total figure implies a

principle of affective opposition or antagonism: There can be a change towards either pole but not a change in opposite directions at the same moment of time.

Although there are two opposed directions of change, there are, logically and psychologically, four main kinds of affective change that need to be considered: (a) increasing positive affectivity, (b) decreasing positive affectivity, (c) increasing negative affectivity, (d) decreasing negative affectivity. The first kind of hedonic change (increasing positive affectivity) is present when an animal tastes a sugar solution and organizes an approach-maintaining pattern of behavior. The fourth kind of change (decreasing negative affectivity) is present when an animal succeeds in relieving the "distress" associated with an electric shock or reducing a need produced by dietary depletion. "Distress reduction" is the hedonic equivalent of "drive reduction" in the organization of instrumental behavior.

Changes in the negative direction occur under various circumstances. When an organism continues eating an acceptable food, the level of acceptability gradually declines as the final state of satiation is approached. Hedonic changes in the negative direction are also produced by shocks, burns, cuts, shrill sounds, and similar conditions. When negative affectivity is present the organism tries to reduce it. The very attempt to escape from inducing conditions is the earmark of negative affectivity.

#### *The Distinction Between Sensory and Hedonic Intensity*

To a psychology that is limited by stimulus-response concepts the postulate of central affective processes may appear superfluous. I believe, however, that any theory of behavior which

ignores the concept of affectivity will be found inadequate as an explanation of the total facts. To prove the point let us consider the following facts which are difficult, if not impossible, to explain in strictly S-R terms:

If pairs of sucrose solutions are presented to rats briefly for choice, the animals select the higher of two concentrations in preference to the lower. Scale values based upon preference tests show that the level of acceptability is directly proportional to the logarithm of the concentration. The relation holds all the way up the intensive scale from the preferential threshold to a saturated solution (Young & Greene, 1953).

From the facts about sucrose solutions one might argue that *sensory intensity* or physical concentration of solution is the critical determinant of behavior. But difficulty with the sensory interpretation appears when one considers the relative palatability of solutions of sodium chloride.

Young and Falk (1956a) ran a series of preference tests between distilled water and sodium chloride solutions of different concentrations, and between pairs of sodium chloride solutions. They found that need-free rats revealed an optimal concentration for sodium chloride within the range of 0.75 to 1.5%. When concentrations were below this range, need-free rats preferred the higher concentration; when above this optimal range, they preferred the lower concentration. Within the optimal range there were marked individual differences in preference and there was much indiscriminate behavior. The experimenters concluded that there is a *range of acceptance* within which acceptability increases with rising concentration of NaCl and a *range of rejection* within which the level of acceptability falls as concentration rises. The optimum

of acceptability appears to be determined by the intersection of two gradients—one of acceptance and one of rejection. Incidentally, this finding agrees well with the work of Bare (1949), and others, who relied upon an intake method of studying acceptability of NaCl solutions.

It is clear, therefore, that with solutions of sodium chloride, hedonic intensity does not have a one-to-one relation with sensory intensity. *Sensory* intensity is an increasing monotonic function of concentration of solution; *hedonic* intensity is a discontinuous function of concentration.

In another experiment, Young and Asdourian (1957) selected a 1% sodium chloride solution as representative of the optimal range of NaCl concentrations. This near-optimal concentration was tested for preference against distilled water and four concentrations of sucrose solutions (54, 18, 6, 2%). The 1% NaCl solution was preferred to distilled water by need-free rats; but *all* sucrose solutions were preferred to the standard 1% NaCl solution. In the study it was estimated that a 1% NaCl solution is isohedonic to a sucrose concentration of 0.38%, which is very near to the preferential taste threshold for sucrose (about 0.50%). In other words, practically all sucrose solutions are more palatable to need-free rats than all NaCl solutions. The hedonic intensity of all NaCl solutions is so low, in fact, that it is impossible to discover isohedonic pairs of sucrose and NaCl solutions.

It is difficult at best to equate sensory intensities across modalities but it is impossible to equate hedonic intensities if we employ solutions of sucrose and NaCl. High concentrations of sucrose are hedonically positive and high concentrations of NaCl are hedonically negative. This finding under-

scores the principle that *sensory* intensity is very different from *hedonic* intensity.

The scaling of hedonic intensities through preference tests yields an order of fact quite different from the facts of sensory intensity. This comes to light clearly in tests (unpublished) by Kent Christensen, who worked with compound solutions containing both sucrose and NaCl. When the sucrose concentration was 1% and the NaCl 0.75% (near optimal), the rats preferred the compound solution to both the 1% sucrose solution and the 0.75% NaCl solution. In other words, the two solutes—sucrose and NaCl—made independent additive contributions to palatability. The experiment demonstrated summation of two positive hedonic intensities. But, to take an extreme example, when the sucrose concentration was 4% and the NaCl concentration was also 4%, the rats showed a marked and consistent preference for the sucrose solution to the compound (very salty) solution. In this instance, positive and negative hedonic values summated algebraically. Christensen demonstrated, therefore, that the palatability of some sucrose solutions can be raised by adding small quantities of NaCl and lowered by adding larger quantities. Although he did not succeed in mapping out isohedonic contours for compound solutions, in an area determined by the concentration of sucrose and the concentration of sodium chloride, he demonstrated their existence and the possibility of charting them.

I believe that these facts demonstrate convincingly the necessity of distinguishing between *sensory* intensity and *hedonic* intensity and hence between the sensory and affective aspects of neural excitation. Stimulation of the taste receptors has both sensory and affective consequences.

### AFFECTIVE PROCESSES AND FOOD ACCEPTANCE

The postulate that affective processes have an objective existence and that they intervene between stimulus and response conditions has great utility in the field of food acceptance. In fact, the experimental findings practically demand such an hypothesis, as we shall see below.

#### *Kind and Amount of Food Accepted vs. Affective Processes*

When an objective psychologist takes a look at food objects he probably sees them as physical things. He observes that food objects differ in *kind* and that the *amount* presented to an animal as a reward or consumed by him is a quantitative variable.

My early studies of food preference utilized common foodstuffs, such as milk, butterfat, sugar, flour, whole-wheat powder, etc. To the human sensorium, as doubtless to that of the rodent, these foods differ in taste, smell, texture, appearance, and other sensory attributes. Several writers have commented that this work demonstrated how performance depends upon the *kind* of food offered as a reward. The statement is doubtless correct but the sensory character of a foodstuff must not be confused with its hedonic value.

I have repeatedly found that, when dietary conditions are held constant, common foods arrange themselves into a preferential hierarchy or transitive series from low to high palatability. The *kind* of food (defined by its physical properties) has no obvious relation to palatability (defined by preference tests). Foods that differ markedly in *kind* may be isohedonic. Also, one and the same *kind* of food may vary greatly in acceptability with such conditions as deprivation and satiation.

The amount or quantity of food ingested has been widely used as a measure of acceptability. Innumerable studies in the fields of nutrition, physiology, and psychology have recorded the amount of food accepted per day by laboratory animals. The quantity accepted per unit of time is certainly a useful measure when related to body weight, growth, reproduction, state of health, and other variables.

The difficulty with quantity accepted as a measure of acceptability came to light clearly in an experiment by Young and Greene (1953). In this study rats were given a 1-hour drinking test with a 9% sucrose solution and again a 1-hour test with a 36% solution. Greater quantities of the 9% solution were ingested consistently throughout the 1-hour test. On the basis of results one might well predict that a 9% sucrose solution would be "preferred" to a 36% solution and that greater quantities of the 9% solution would be ingested if the animals were given a choice between the two fluids. This prediction, however, turned out to be contrary to fact. In a test with two cups, the rats ingested greater quantities of the 36% solution. Also, on the brief-exposure preference tester, the 36% solution was selected in preference to the 9% solution.

The nature of the difficulty is clear. Intake depends upon two kinds of determinants—palatability and appetite. Hence intake is an ambiguous measure of motivation.

#### *The Distinction Between Palatability and Appetite*

The term *palatability* refers to the hedonic value of a foodstuff that depends upon taste, aroma, texture, temperature, appearance, and other sensory properties, and upon the surroundings of a foodstuff (environmental setting). In need-free organisms

(which have been studied extensively during the past few years), a preference between two taste solutions reveals a difference in palatability.

The term *appetite* refers to internal determinants of food acceptance and preference. Insofar as a preference is determined by deprivation or satiation, by surgical operation, by special conditions such as pregnancy and lactation, by diseases, etc., it is appropriate to speak of appetite. When an animal eats a food continuously there are appetitive changes. There is some evidence for a "warming up" effect which the French have expressed in an aphorism: "L'appétit vient en mangeant." And with continued eating there is the inevitable approach to satiation which is experienced subjectively as a reduction of desire.

The distinction between palatability and appetite does not imply that there are two kinds of affective processes. I would postulate only one kind of affective process, with positive and negative signs, but different sets of conditions that affect it.

An experiment by Shuford (in press) will be cited to illustrate the validity of the distinction between palatability and appetite as objective concepts.

Shuford studied the relative acceptance of sucrose and glucose solutions by need-free rats. He arbitrarily selected for study three concentrations of glucose solutions, namely, 5 and 15 and 35%. For each of these solutions he computed, on the basis of preference tests of the intake type, the concentrations of sucrose solutions that would be equally acceptable.

During the main experiment 30 animals were given, individually, a 20-minute drinking test with each of the six sugar solutions (three of glucose and three of sucrose) presented singly. Each rat was given one test per day. The six solutions were presented ac-

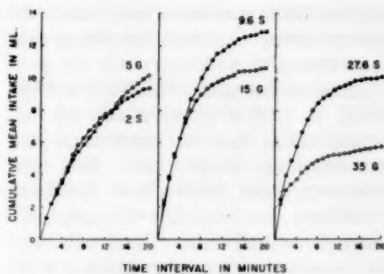


FIG. 2. Intake curves for pairs of isohedonic solutions of sucrose (S) and glucose (G). From Shuford (in press).

cording to a counterbalanced latin square design.

The curves presented in Fig. 2 show the cumulative mean intake for each of the six solutions as a function of drinking time. It is obvious at a glance that the six solutions do *not* yield identical curves of intake. The greatest quantities of fluid consumed during the 20-minute test were for the 9.6% sucrose solution (9.6 S) and the 15% glucose solution (15 G). The lowest fluid intake was for the pair of solutions with the highest concentrations (27.6 S and 35 G).

A careful study of these three pairs of curves reveals an interesting fact. The *initial* rate of acceptance is practically the same for the two fluids in each pair of isohedonic solutions. Compare the curves for the first 12 minutes in the tests with 5 G and 2 S; and the curves for the first 5 minutes in tests with 15 G and 9.6 S; and those for the first 1 or 2 minutes in tests with 35 G and 27.6 S. The *initial* rates of acceptance are nearly identical for each pair of solutions. If the initial slopes of these pairs of curves are studied by noting the angle of a curve to the vertical, it will be seen that the highest rate of acceptance was for the pair with highest concentrations; an intermediate rate was obtained for the pair with

intermediate concentrations; and the lowest initial rate was for the lowest concentrations.

In an earlier study, McCleary (1953) failed to find a positive relation between initial rate of acceptance and concentration of solution. His rats, however, were thirsty and doubtless drank at a maximal rate of acceptance throughout the tests. Shuford's rats, by contrast, were neither thirsty, hungry, nor deprived in any known way, and their initial rate of ingestion was positively related to the concentration of solution.

Shuford's work confirms a distinction, drawn by McCleary, between a taste factor and a postingestion factor in the regulation of intake. The taste factor regulates the initial rate of ac-

ceptance; but sooner or later the post-ingestion factor checks the intake. Both McCleary and Shuford demonstrated that the checking of ingestion as the limit of satiation is approached is regulated in some way by the osmotic pressure of the fluid contents of the stomach.

Figure 3 presents Shuford's data plotted to show the influence of osmotic pressure upon intake. Osmotic pressure (in atmospheres) is represented along the baseline which shows the location of the four sugar solutions that are hypertonic. The illustration gives cumulative mean intake (in cc.) for the four hypertonic sugar solutions, plotted after 1, 4, 8, 12, 16, and 20 minutes of drinking.

It is obvious from Fig. 3 that the quantity of fluid ingested during a fixed period of time is a decreasing function of osmotic pressure of the solution as well as an increasing function of the duration of drinking time. If the drinking time is held constant, at 4 or 8 or 20 minutes, the quantity of fluid ingested decreases as the osmotic pressure increases.

In general, this experiment demonstrates the validity of the distinction between the peripheral (sensory) and internal (organic) determinants of intake. Since both sets of determinants regulate intake, it would be confusing, as I pointed out above, to interpret the quantity of fluid ingested as an index of either appetite or palatability. The two groups of determinants operate jointly but they can be experimentally distinguished.

#### *Two Effects of Deprivation*

One of the commonest techniques for controlling the degree of motivation is to deprive an animal of food or water for a specified period of time. Thus one reads about a 24-hour hunger drive or a 12-hour thirst drive. Well-starved

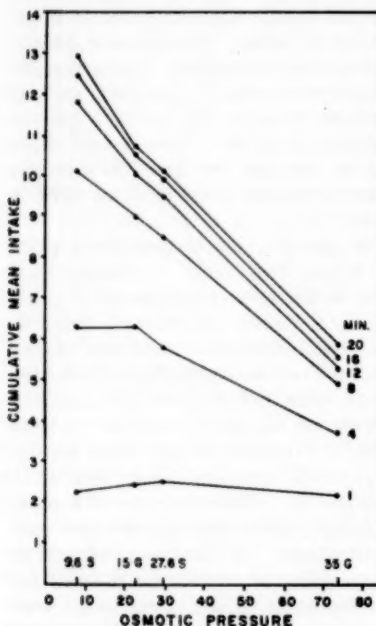


FIG. 3. Cumulative mean intake of sucrose (S) and glucose (G) solutions as a function of osmotic pressure and drinking time. From Shuford (in press).

animals are the rule rather than the exception!

One limitation of the starvation technique is that with prolonged deprivations an enfeeblement develops which lowers the level of drive. Although *drive* is low, the *need* for nutrient increases steadily up to the point of death through inanition. Doubtless incentive value increases, too, and this is not necessarily the same as drive.

For several years I have experimented with need-free rats, i.e., rats that have an unlimited supply of adequate diet and water in their cages at all times up to the moment of testing. There can be no doubt that such need-free animals respond positively to food incentives that are supplementary to their maintenance diet just as a child responds positively to candy which he does not need.

I have controlled degree of motivation by manipulating incentive conditions rather than by deprivations. When solutions are employed as incentives, a precise quantitative control over motivation is possible and, incidentally, it is possible to scale incentives with a precision far exceeding that obtainable from the starvation methods.

In two experiments a 1-day thirst was introduced for experimental purposes. Two interesting side effects of deprivation were discovered more or less accidentally:

1. Young and Falk (1956a) found that thirst made rats less ready to discriminate between two kinds of drinking water. Nonthirsty animals, previously trained to run upon the preference tester, were given a choice between local tap water and distilled water. All 12 animals selected tap water in preference to distilled; 5 of the 12 revealed a preference significant at the .01 level or better; no rat preferred distilled water. When the rats

were made thirsty this preferential discrimination vanished. The rate of running on the apparatus was markedly increased by thirst but, as we said, the rats became indiscriminate.

Our interpretation of this finding was that thirst raised the appetitive level for all kinds of drinking water. The appetitive increment masked a small but consistent difference in palatability which was readily demonstrated under need-free conditions.

2. In a related experiment Young and Falk (1956b) were studying the relative acceptability of sodium chloride solutions. It was found that need-free rats occasionally accepted sodium chloride solutions with concentrations as high as 3 or 6%. We hoped that we could force these rats to show a preference between pairs of hypertonic NaCl solutions by making them thirsty. Actually, we found that a condition of thirst inhibited the running activity when the NaCl incentives were 3 and 6%.

This inhibition remained when the thirst was removed and a more palatable solution substituted for one of the hypertonic solutions. Some of the animals eventually recovered normal running but they recovered it via sporadic bursts of activity at a high rate of running—a fact which indicates that thirst merely inhibited a well-practiced running pattern. There was *inhibition* of the shuttle pattern rather than *extinction* of the kind that occurs when sucrose is withdrawn from an incentive fluid.

In general: (a) Deprivation may obscure a small but consistent difference in palatability that is obvious under nondeprivation conditions. (b) Deprivation may inhibit activity if the starvation technique is used as a means of forcing animals to discriminate between two hedonically negative test-fluids.

### *The Experimental Control of Food Preferences*

The techniques employed for the experimental control of food preferences and some of the main results obtained will be summarized under five headings:

1. *Reversal of a preference through satiation upon the preferred food.* In one experiment (Young, 1940) it was determined that the maintenance diet supported a stable preference of sucrose to whole-wheat powder. Then rats were prefed upon sucrose, for controlled periods of time, immediately before tests of preference. Prefeeding to the extent of 100–400 contact-seconds did not change the preference but, when the animals were satiated (900 contact-seconds or less), all of them developed a preference for whole-wheat powder.

The preference did not change immediately, but it developed gradually through a series of choices. When the prefeeding of sucrose was discontinued the original sugar preference did not immediately return, but there was a gradual and unmistakable trend in the direction of the original preference.

2. *Reversal of a preference through creating a metabolic need for the non-preferred food.* In an experiment by Young and Chaplin (1945) groups of rats were maintained in cafeteria cages upon a self-selected diet, i.e., the animals were given unlimited access to the components of an adequate diet, presented in separate containers. Repeated preference tests were made between two of the dietary components: sucrose and casein. The need-free rats developed a consistent preference for sucrose. After the preference had developed, protein starvation was commenced by removing casein from the diet.

As protein starvation steadily increased, the animals continued to select

the sucrose (which they did not need) in preference to the casein (which they obviously did need). Then a new technique of preference testing was introduced. The rats were placed in an apparatus that resembled a Y maze with test-foods far apart and in fixed, familiar positions. In this apparatus all animals promptly developed a preference for casein. For a while there were two incompatible preferences. On the standard apparatus the rats showed a preference for sucrose; on the Y-maze apparatus they showed a preference for casein.

A series of control experiments (Young, 1945) demonstrated that if naive rats are first depleted of protein and then tested with both kinds of apparatus, they develop the same preference—a preference for casein. This preference agrees completely with metabolic needs.

The total investigation demonstrates these principles: (a) Need-free rats prefer sucrose to casein and the difference is one of palatability. (b) Protein-starved rats develop a preference of casein to sucrose, in agreement with their metabolic needs. (c) Established habits of preference tend to persist as regulators of food selection even when the selections are out of line with metabolic needs and out of line with previous relations of palatability.

3. *Control of preference by changing palatability.* An obvious technique for controlling food preference is to alter the palatability of one of the test-foods. Humans alter palatability whenever they add sugar or salt or vinegar to a food or change its temperature to make it taste better. Actually this is a change in the character or kind of food.

In current work with compound solutions it has been found that the addition of sodium chloride to a sucrose solution may either raise or lower the level of palatability depending upon the

concentrations of both components. The employment of compound solutions makes possible a great deal of objective research upon the determinants of relative palatability.

4. *Control of preference by associating a flavor with organic relief.* During the early years of vitamin research, Harris et al. (1933) studied the acquisition of preferential food habits by laboratory rats. They found that when rats were depleted of the Vitamin B complex, they were unable to select a food containing a small but sufficient amount of the vitamin. If, however, the animals suffering from avitaminosis were isolated and fed a vitamin-adequate food with a distinctive flavor, so that they could associate a specific flavor with relief from the distressing symptoms of vitamin deficiency, they continued to select this food when it was placed among other foods. The rats continued to select this food (labeled by a flavor) even after the vitamin was withdrawn and the diet again made inadequate.

This pioneer experiment indicates that preferential food habits can be altered by associating sensory qualities with organic relief from distress. It also supports the principle that food habits, once they are formed, take over the function of regulating food selections independently of need and affective processes.

5. *Control of preference through habituation and training.* Experiments upon the control of food preference through training, when palatability and metabolic need are held constant, have thus far given negative or indecisive results. In one study rats were forced to accept casein (an unpalatable food) for 1,000 runs without choice and periodically, during their training, given brief preference tests between casein and sucrose. The forced acceptance of an unpalatable food only made the

preference for sucrose stand out with increasing clarity and certainty.

In general, the techniques that have proved most successful in the experimental control of food preferences have typically involved an hedonic change in the positive direction. And there is much evidence that, once a food habit has taken over the regulation of the feeding process, this habit tends to persist as a stable determinant of food selections regardless of metabolic needs and prior palatability relations.

#### LEARNING AND PERFORMANCE AS RELATED TO THE AFFECTIVE PROCESSES

##### *Reinforcement Defined in Terms of Probability*

What is reinforcement? According to my dictionary, reinforcement is something which strengthens. An army is reinforced by bringing up fresh troops, ammunition, and food; concrete is reinforced by addition of metal rods.

For a good many psychologists reinforcement is a kind of strengthening of associative bonds that is revealed by an increased probability that a given stimulus-situation will elicit a specific response. Thus Spence (1956) writes:

Environmental events exhibiting this property of increasing the probability of occurrence of responses they accompany constitute a class of events known as *reinforcers* or *reinforcing events*. All environmental events not exhibiting this property fall into a different class that may be designated as *non-reinforcers*. . . . Responses accompanied or followed by certain kinds of events (namely, reinforcers) are more likely to occur on subsequent occasions, whereas responses followed by certain other kinds of events (namely, nonreinforcers) do not subsequently show a greater likelihood of occurrence (Spence, 1956, pp. 32-33).

A definition of reinforcement in terms of probability is immune to criticism because it simply generalizes the

empirical facts. This type of definition is theoretically safe. Its limitation is that it tells nothing concerning the nature of the reinforcing event. We would honestly like to know what goes on within the organism when reinforcement occurs and what goes on when a learned response becomes extinguished.

When learning is viewed in terms of probability one fact stands out clearly: Exercise (practice, drill, training) results in an increased probability that a given stimulus-situation will elicit an associated response, and the lack of exercise results in a decreased probability. Most psychologists, however, would distinguish between learning through exercise and reinforcement; and most would distinguish between extinction and forgetting.

To clarify the picture let us first regard reinforcement as a phenomenon of learning that is due to exercise and then consider another view.

*Reinforcement as Growth of Habit  
Strength Through Practice*

Hebb (1949), following Lorente de Nó, assumed that the physical basis of learning is a growth of synaptic knobs. When the axon of Neuron A excites Neuron B repeatedly and persistently, there is a growth process in one or both of the nerve cells such that A's efficiency in firing B is increased. This growth process appears as the development of synaptic knobs on one or both of the neurons at the locus of functional contact. The number and size of these knobs are dependent upon the frequency of joint excitation. Presumably the area of contact is the decisive factor in determining the likelihood that activity in one cell will fire another. The greater the area of contact, the lower the synaptic resistance. According to this view, learning is a physical growth process dependent upon simultaneous excitation of two

or more contiguous nerve cells. Learning is a physical change in neural structure. A frequency-contiguity theory of learning appears sufficient to explain the facts of practice.

The view that practice reinforces an S-R bond and lack of practice extinguishes it, is unacceptable to most psychologists because it appears to miss the point. This view is out of line with the formulations of Hull, Skinner, Spence, Sheffield, and others, as well as with Pavlov's original meaning.

Pavlov demonstrated that, for a conditional reflex to persist, it must occasionally be "reinforced." Everyone knows that if the bell is sounded repeatedly and the dog is never rewarded by meat powder, the CR becomes "extinguished." For a CR to continue, it must occasionally be "reinforced" by the presentation of food.

A good many psychologists describe reinforcement not as an organic event but in terms of the reinforcing agent—food—but food, of course, is only one kind of reinforcement. In going over some recent literature upon reinforcement I noted the following statements: Reinforcements are weighed in grams, measured in cubic centimeters, counted out in uniform pellets, controlled by drops of fluid. The concentration of sugar in a solution measures the *amount* of reinforcement. Some reinforcements are solid; others are fluid. One writer stated that the reinforcements were consumed by the rat! Reinforcements differ in amount and in quality; their presentation can be delayed, placed upon various kinds of schedules, etc. Now I submit that these statements make sense if, and only if, we assume that reinforcement is a food object offered to the animal as a kind of reward. There is thus a rather careless use of the term *reinforcement* and a tendency to ignore its nature as an internal, organic, process.

*Reinforcement as Change in Performance Dependent upon Affective Processes*

According to another view, reinforcement is a determinant of *performance* rather than *learning*. Performance depends upon affective processes in a way that can be demonstrated with good precision. Two experiments will be described to illustrate the point:

1. Young and Asdourian (1957), in the experiment considered above, offered rats a choice between a 1% sodium chloride solution and a sucrose solution having a fixed concentration. For one group of animals the sucrose concentration was 54% (very sweet); for a second group the concentration was 18% (definitely sweet); for a third group the concentration was 6% (weakly sweet); for a fourth group the concentration was 2% (very weakly sweet). There were 8 rats in each group. Every animal was given a total of 100 choices between a sugar solution and the standard solution of 1% salt.

The 32 rats uniformly developed a preference for the sugar solution rather than the salt. The rate of growth of this preferential discrimination, however, depended upon concentration of the sugar solution.

Figure 4 shows the proportion of choices of sugar solution when tested against a standard salt solution. The proportions of choices for each sucrose solution have been computed on the basis of cumulative choices. To be specific: the first proportions are based upon the first 10 choices per rat; the second set of proportions are based upon the first 20 choices per rat; the third upon the first 40 choices per rat, and so on. The last proportions are based upon the total series of 100 choices per rat (800 for each experimental group).

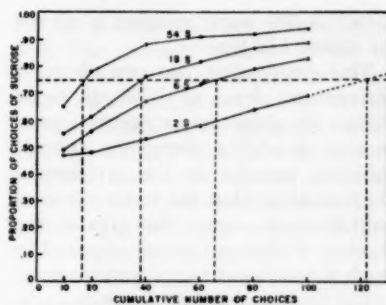


FIG. 4. Proportion of choices of sucrose solutions, having the concentrations shown, when paired with a standard solution of 1% sodium chloride. The curves are plotted from data obtained by Young and Asdourian (1957).

Figure 4 shows that the proportion of choices of a sugar solution depends upon the total number of previous choices. This is because the animal steadily *learns* to make a preferential discrimination. The figure clearly shows that the growth of a preference is dependent upon two factors: (a) amount of previous practice, and (b) concentration of solution. Now, in terms of the probability doctrine, one must recognize that the probability that a rat will select a sugar solution in preference to a standard salt solution increases with both factors.

To illustrate the point further, consider a probability of 0.75—midway between chance (0.50) and invariable preference (1.00). In Fig. 4 a line of dashes, parallel to the base, has been drawn at 0.75. Auxiliary dash lines have been added to indicate that sugar will be selected in preference to salt in 75% of the choices if:

- (a) A 54% sugar solution is offered for choice 17 times.
- (b) An 18% sugar solution is offered for choice 38 times.
- (c) A 6% sugar solution is offered for choice 66 times.

(d) A 2% sugar solution is offered for choice 122 times.

This means that one can obtain a performance level of 75% of sugar choices for almost any concentration of sucrose simply by giving the animals sufficient practice in the preferential discrimination; but the lower the concentration of sugar, the greater the number of choices that are required to reach a specified level of performance.

The two factors that determine performance are independently variable and each can be expressed in equivalent terms of the other. It is possible, for example, to determine the *practice equivalent* of a 10% increase of sucrose concentration.

2. Another experiment that is closely related to the above has been described by Dufort and Kimble (1956). Their work clearly shows the relation between performance and the concentration of sucrose solution offered as a reward.

In their experiment rats ran down a straight 18-inch runway to a semi-circular goal platform around the circumference of which were five cups (bottle caps) equally spaced. During pretraining, all animals received one drop of a 10% sucrose solution in each of the five cups. Then, during an

original learning period of five trials, the animals learned to run to one of the cups that contained a single drop of sucrose solution, the other four cups being empty. Following this original learning, the 40 animals were divided into four equivalent groups of 10 rats each that were treated differently. The rats in one group learned to run to a cup containing a single drop of 20% sucrose solution. Those in a second group learned to run to a cup containing a single drop of 10% solution, i.e., there was no change of concentration. A third group learned to run to a cup containing a single drop of 5% solution. A fourth group was tested with a drop of distilled water (0% sucrose) in one of the cups.

Results are presented graphically in Fig. 5. Dufort and Kimble plotted the mean percentage of correct responses under the different incentive conditions. It is clear from their data that the mean percentage of correct choices is a direct function of the concentration of sucrose within a single drop of incentive fluid. The 20% solution gave better performance than the 10%; the 10% solution was better than the 5%; the distilled water (0%) led to extinction of the previously learned pattern. Dufort and Kimble correctly regard extinction as a limiting case in a series of tests with reduced concentrations of sucrose.

The extinction of a learned pattern when distilled water was substituted for a sucrose solution recalls Guttman's (1953) concept of the reinforcement threshold.

This also recalls an unpublished study in which it was found that learning occurred intermittently when incentives of low hedonic value (2% and 6% sucrose solutions) were offered to need-free rats. The animals were timed as they made 1 run per day down a 5-foot straightaway. Some animals

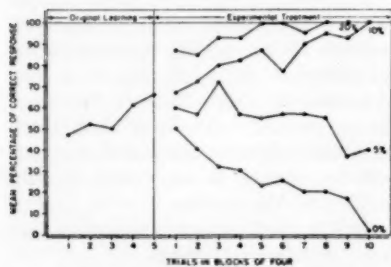


FIG. 5. Mean percentage of correct responses as a function of practice and incentive value. Original data are for 40 rats. For each experimental treatment,  $N = 10$ . From Dufort and Kimble (1956).

showed learning for a series of days and then for a long series of days they continued to approach the sugar solution but with no indication of learning. They showed no improvement with practice; their performance was on a plateau. Animals running to higher concentrations, by contrast, produced the usual type of performance curve with negative acceleration.

It is clear from the above experiments that reinforcement may mean either of two things: (a) Reinforcement is a phenomenon of learning—a change in habit strength due to practice. (b) Reinforcement is a phenomenon of motivation—a change revealed in performance that is dependent upon affective processes associated with sensory stimulations. The second meaning will be accepted for purposes of the present discussion.

#### MOTIVATION AND THE AFFECTIVE PROCESSES

The study of motivation is notorious for its unsettled state. The difficulty lies in basic concepts and definitions. Just what does the study of motivation include?

If motivation is viewed as a process, there are at least three aspects that must be considered in defining the concept. First, motivation may be viewed as the process of arousing and sustaining action. This includes physiological activation through sensory stimulation, inner chemical sensitization, direct brain action, and through other means. Second, motivation may be viewed as the process of regulating and directing behavior, especially channeling behavior toward goals. This aspect includes regulation through neural structures, direction by purposive sets and orientations, and regulation by environmental conditions. Third, motivation may be viewed not as a contemporary event

but as a disposition or psychological state that persists over a period of time. This view implies the existence of a stable organism which carries about with him certain attitudes, evaluative dispositions, intents to act, interests, and sentiments, as well as latent abilities, skills, and habit structures.

How do affective processes fit into this comprehensive picture of motivation? I think that from every point of view the affective processes must be regarded as motivational in nature. First, affective processes activate neurobehavioral patterns. Second, affective processes regulate and direct behavior according to the principle of maximizing the positive and minimizing the negative. Third, affective processes have the specific role of organizing neurobehavioral patterns. They lead to the development of motives and evaluative dispositions that are relatively stable and permanent. How they accomplish this is a question for research and not speculation.

#### *Affective Intensity and the Strength of Motives*

In earlier studies I wrote about "food-seeking drives," but this terminology is confusing, since food-seeking behavior is *learned* and drives are *innate*. Judson Brown (1953) deserves the credit for pointing out the ambiguity in the concept of acquired drives. The term *drive* has many meanings, and I would like, in this paper, to sidestep the problem of defining drive. If we follow Nissen (1954), all drives are innate and it is nonsense to talk about acquired drives. Animals acquire patterns of behavior that are instrumental in arriving at goals. Rats learn to run a maze, to press a bar, to shuttle back and forth on the preference tester, to swim over a pathway, etc., in order to arrive at a goal. What they learn

is not a drive; it may, with equal propriety, be called a habit or a motive. By common consent it is proper to speak of the acquisition of motives since *motives*, by definition, are learned and not innate.

Several experiments will be considered to show how the strength of a motive depends upon the intensity of affective processes:

1. In one experiment (Young, 1947) rats were timed as they ran back and forth upon the preference tester to obtain nibbles of food from a single cup. Each rat made 30 runs (round trips) for a sucrose incentive, 30 for wheat, and 30 for casein. The total of 90 runs constituted a cycle, and each animal went through the cycle five times.

Preference tests established the fact that sucrose was the most palatable of the three incentives; wheat was almost as palatable as sucrose; casein was very low in palatability relative to the other two incentives.

The data showed that the rate of running was directly proportional to the palatability level of the incentive. The animals ran fastest for a sucrose incentive, almost as fast for wheat as for sucrose, and the slowest running was for casein. The rate of running, therefore, correlated perfectly with the level of palatability as determined by preference tests.

Running, however, is not learning. The rate of running is known to vary with many factors other than palatability and there are wide individual differences in running rates. To equalize these gross differences in running rates and to discover the dependence of performance upon practice alone, I arbitrarily equated the total time required by a rat to complete 30 runs, for a given incentive, with unity. Then I divided this unitary (total) time into thirds to discover how many runs were completed during each successive third.

There were practice gains with every incentive, but a careful study of the data failed to reveal any significant differences among the practice gains with the three incentives. Learning appeared to depend solely upon the amount of practice in running the shuttle pattern and not at all upon the palatability of the incentive.

It was concluded that learning is a function of the number of runs, i.e., the amount of practice, quite apart from the palatability of the incentive. At equivalent stages of learning, of course, the rats ran faster for the more palatable incentive. But running is *performance* (depending upon motivation, learning, and other factors) and is not to be confused with a pure practice effect.

2. The dependence of strength of motives upon hedonic intensity, duration, and repetition was studied in an experiment by Young and Shuford (1954). To control hedonic intensity they varied the concentration of sucrose solutions (54 or 18 or 6 or 2%); to control hedonic duration they varied the number of seconds of contact with a solution per trial (1 or 4 or 16 contact-seconds); to control repetition, or frequency of affective arousal, they gave the animals one run per day for 18 days.

In the main experiment each rat had had opportunity to explore the total apparatus and also 60 seconds of preliminary contact with his specific sucrose incentive in the cup, prior to Trial 1. On Trial 1 the approach time from starting box to test-fluid was measured. It was found that the rats ran faster in approaching a sucrose solution of 54% or 18% than to one of 6% or 2%. In other words, rats ran faster to a highly palatable incentive than to one of low palatability.

All animals speeded up markedly with practice. After 14 to 18 daily

runs the rats in all incentive groups approached the sucrose solution with near-maximal speed. The initial differences in speed of running (dependent upon palatability) were obscured by practice which had the effect of accelerating all animals and of reducing group differences in performance.

This finding suggests that positive affective processes have the role of organizing approach motives. After an approach motive has been organized it tends, with practice, to become automatic and habitual. In other words, habit takes over the initial regulatory function of the affective processes.

In this experiment a positive relation was found between approach behavior and the duration of affective processes. The experiment was so designed that one-third of the animals in each incentive group received daily 1 contact-second with the sucrose solution; another third received 4 contact-seconds; another third received 16 contact-seconds. After practice, the *latency* (time required for a rat to leave the starting box) was inversely related (.05 confidence level) to the duration of contact with the sucrose solution.

3. In another study, Mason (1956) controlled the duration of contact with a sucrose solution by varying the number of separate licks. He found that 50 licks of sucrose solution were more effective than 10 licks as an incentive to rats running down a 5-foot straight-away. He found also, in agreement with Young and Shuford, that the concentration of sucrose solution and the frequency of contact (which he described as the ratio of reinforced trials) were highly significant determinants of performance.

The above three investigations agree completely in the main points. All of them show that the intensity of affective arousal (controlled by kind of food or by concentration of sucrose solution),

the duration of affective arousal (controlled by number of contact-seconds or number of licks), and the frequency of affective arousal (controlled by number of contacts with the food) are significant determinants of the rate of running for a food incentive.

The data justify the postulate that the *strength* of a motive to approach, as shown by speed of locomotion and latency, is directly related to the intensity, duration, and frequency of affective arousals. We might add that the recency of affective arousal is probably also a factor.

It should be kept in mind that in the Young and Shuford experiment the rats had only one brief contact with the sucrose solution per day and that the strength of an approach motive was measured 24 hours after this brief contact. This means that there are at least two factors in the motivation: the primary affective arousal and the learned neural determination that orients the animal toward the goal and regulates his approach. The learned orientation is an approach motive and its strength is correlated with hedonic intensity, duration, frequency, and probably recency.

It is obviously necessary to distinguish between the primary hedonic motivation and the secondary motivation that is based upon it. I would postulate that, after an initial contact with the sucrose solution, the environmental stimulus cues from the apparatus re-integrate the animal's goal orientation and that the strength of this secondary motivation is dependent upon the intensity of proprioceptive tension produced by the goal-orientation. This means that proprioceptive tension in the learned motivation is positively correlated with hedonic intensity in the primary motivation. This is a matter, however, which should be tested in the laboratory and not settled by words.

*The Regulatory Role of Affective Processes in Preferential Learning*

Preferential learning differs from the acquisition of a simple running motive in that it implies an element of choice. If a rat, on the preference tester, develops a preference of  $A > B$ , I would assume that A is more palatable than B; if he develops a preference of  $B > A$ , I would assume that B is more palatable. If no preference develops, there is no basis for assuming a difference of palatability; the two test-foods may be isohedonic or the animal may be indiscriminate.

A preference test does not reveal the absolute level of affective intensity but only the relative levels associated with two incentives. This fact may be illustrated by curves plotted from data in which preference tests were run with three pairs of test-foods: (a) sucrose and casein, (b) wheat and casein, and (c) sucrose and wheat (see Young, 1947, p. 49). The percentages that indicate preference were highest for the first pair (sucrose and casein), where the difference in palatability was greatest. The percentages were lowest for the third pair (sucrose and wheat), where the difference in palatability was slight but the incentives themselves were of high palatability. A preference test, therefore, reveals only the relative affective intensities.

This same principle of relativity can be illustrated by the data presented in Fig. 4. In the experiment (described above) a 1% NaCl solution, used as a standard in all comparisons, turned out to be very low in palatability. Consequently, the difference in affective intensity between the NaCl standard and a sucrose solution is proportional to the concentration of the sucrose solution. The curves show clearly that the rate of development of a preferential discrimination is dependent upon

the difference of hedonic intensity between the two incentives. Incidentally, these growth curves are not true curves of learning. They are curves of performance in which both learning and affective processes contribute to the growth process.

In the light of many such sets of curves, I make the assumption that affective processes are regulative and organizing in function. The relative intensities of affective arousals associated with two stimuli determine whether the animal will develop one preference or its opposite or no preference at all. Also, the relative hedonic intensities determine the rate of growth of a preferential discrimination when practice is held constant.

Because of their regulative role the affective processes are importantly related to learning as well as to performance. Affective processes determine what neurobehavioral patterns an animal will organize and exercise, whether or not he will learn a particular pattern, how many trials it will take him to learn a pattern up to some specified criterion of performance, how well he will perform a learned act, etc. But despite all of this, it is impossible to equate affective processes with learning. I know of no evidence that affective processes cause learning. They have a regulative and organizing function and hence influence the neurobehavioral patterns that are learned. Affectivity is not necessary for learning to occur; it is certainly not necessary for human learning.

*Incentive Value and Affective Process*

If a rat shows a positive orientation toward a sugar cup, this orientation can be taken as a mark of positive evaluation. If an animal shows a negative orientation toward a charged grill, this orientation can be taken as a negative evaluation. Similarly, if an ani-

mal develops a preferential habit, consistently selecting A rather than B, his consistent choices indicate a relative evaluation of two incentive objects.

Evaluative dispositions develop through experience; they are based upon affective processes but they have a stability and permanence (dependent upon the nervous system) that is independent of current affective arousals. The sign of an evaluative attitude is based upon the sign of the affective process aroused by the object. The degree of incentive value is related to the intensity of primary affective arousal.

Incentive values can be changed by deprivation and satiation. For example, an unpalatable food, like casein, can become more highly valued than sucrose if rats are starved for protein. Again, the incentive value of drinking water is greatly raised by thirst and lowered by satiation upon water.

An evaluative disposition may be shown in behavior when there is no contact with the incentive object, as an experiment by Schlosberg and Pratt (1956) demonstrated. They baited one side of a T maze with food which could be smelled and seen by the rats but not eaten; the food was covered with a mesh screen so that it was definitely inaccessible. Rats learned to run the maze under conditions of hunger; they did not learn it under conditions of food satiation. The only incentive in this experiment was the sight and smell of a familiar food. There was no consummatory response, no drive reduction, no affective arousal through contact with the food. It might be said that when animals are hungry, food objects—even though inaccessible—are interesting and exciting. They have incentive value.

This finding recalls an early experiment by Grindley in which hungry chicks ran down a runway to grains

of rice which they ate; other chicks ran down the same runway to a plate glass through which they could see, but not touch, the grains of rice. The sight of food was temporarily effective as an incentive but after the first 4 or 5 trials the inaccessible food reward began to lose its motivating effectiveness.

In general, it must be recognized that incentive value cannot be identified with affective arousal, but incentive value has an obvious dependence upon affectivity. Incentive value is based upon an acquired (neural) disposition which has been organized through affective experience. It would be premature, as Koch (1956) would certainly agree, to assert that affectivity is the *only* basis upon which evaluative dispositions are formed. The nature of incentive value needs further study.

#### *Physiology of the Affective Processes*

Recent investigations in physiological psychology indicate that affective processes have their locus in subcortical regions of the brain. The implications of the hedonic continuum (Fig. 1) agree well with findings of current research upon "reward" and "punishment."

Olds (1955) has shown that if electrodes are implanted within the rat's brain, especially within the septal area of the limbic system, electrical stimulation of these points is "rewarding." For example, if a hungry rat is stimulated in a "reward" center when he approaches food, he will stop in his path at the point where he receives internal stimulation. Certain tracts in the subcortical region appear to be related to "reward": they have been called "pleasure centers."

In a series of experiments by Delgado, Roberts, and Miller (1954), the electrical stimulation of certain points within the brain of the cat appears to

act like "punishment." For example, cats learned to rotate a wheel at the sound of a buzzer when rotation of the wheel broke a circuit and discontinued stimulation of the "punishment" centers through implanted electrodes. These and other investigators have also reported neural locations which, when stimulated, are neither "rewarding" nor "punishing."

Miller (1957) reported some observations by Roberts, on cats and rats, indicating that both "rewarding" and "punishing" effects can be elicited from stimulations of the same neural points. A rat, for example, would press a bar to produce electrical stimulation at a point in his brain, then go to the other end of the apparatus and rotate a wheel to turn off the current. The sequence of turning the current on and off was repeated many times. Obviously one must know more about this phenomenon factually before venturing an interpretation. But apart from this, the total evidence indicates that direct electrical stimulation of certain sub-cortical points is "rewarding"; stimulation of other points is "punishing"; stimulation of still other points is neither or possibly both "rewarding" and "punishing," depending upon conditions.

In the light of these important discoveries within physiological psychology, it is reasonable to assume that certain peripheral organs have built-in connections with the neural mechanisms of affective arousal. The taste receptors for sweet, for example, appear to be connected with positive hedonic mechanisms, for all intensities of sweet are acceptable to the rat and to most other animals. The receptors for salt, by contrast, appear to produce weak positive or neutral affectivity at hypotonic concentrations but negative affectivity at hypertonic concentrations. This fact suggests that the same cen-

tral mechanisms can mediate positive or negative affectivity, depending upon the intensity and possibly the duration of stimulation.

This physiological work raises a lot of unanswered questions. It is important to discover how "reward" and "punishment" are related to activity within the reticular formation and to activation on the cortical level. It is important, also, to discover how affective processes are related to activity in specific neural centers such as those that regulate sexual behavior, eating, drinking, sleeping, etc.

One point is certain: It makes sense today to speak objectively about the physiology of affective processes.

#### OBJECTIVE PRINCIPLES OF EXPERIMENTAL HEDONISM

Some of the objective principles of experimental hedonism are stated briefly below. These statements should be regarded as tentative formulations and as a basis for further experimental studies:

1. *Stimulation has affective as well as sensory consequences.* Along with gustatory stimulation by sugar solutions, for example, there is a positive affective arousal which, by its very nature, is something to be prolonged and intensified. Along with painful stimulation, there is a negative affective arousal which, by its very nature, is something to be avoided.

2. *An affective arousal orients the animal toward or against the stimulus-object.* This orientation can be readily observed. For example, when a rat, in the course of exploratory activity, makes contact with a sugar solution he may pause for a moment, then continue to explore. Sooner or later, however, he returns to the solution and takes more. After repeated sips he becomes oriented toward the solution. If an experienced animal is delayed in

his approach to the cup, he shows a postural orientation toward the cup and approaches it quickly when released. If, however, the animal is offered a quinine solution, he fails to develop a positive orientation or an existing positive orientation is inhibited.

3. *Affective processes lead to the development of motives.* An orientation toward the goal-object is a motive that instigates and regulates behavior. The sign of an affective arousal determines whether an approach-maintaining motive or an avoidance-terminating motive will develop. This principle can be illustrated by numerous runway experiments in which animals acquire, through affective arousals, motives that lead to approach or to avoidance.

4. *The strength of a recently acquired motive is correlated with the intensity, duration, frequency, and recency of previous affective arousals.* On the positive side, at least, the speed with which need-free rats approach a sucrose solution is related to the concentration and to the duration, frequency, and recovery of contact. With practice, however, animals speed up their running as they approach a physiological limit. This speeding up with practice may obscure initial differences due to affective arousals.

5. *The growth of motives is dependent upon learning as well as upon affective arousals.* Learning of a simple pattern such as running down a straight alley or running back and forth upon the preference tester is dependent directly on exercise (practice, drill, training); but affective arousals play an essential motivating role in the organizing, activating, regulating, and sustaining of neurobehavioral patterns.

It is necessary, therefore, to distinguish between learning through exercise (practice, drill, training) and the hedonic regulation of behavior. Affective processes regulate and organize

neurobehavioral patterns in the sense that they determine what will be learned and what not; but such hedonic regulation and organization are not to be confused with learning through practice. Learning is here defined as a change in neurobehavioral pattern that depends upon exercise. Affective processes do not *cause* learning. They are motivational in nature and they influence performance.

6. *The laws of conditioning apply to affective processes.* Psychologists ordinarily describe conditioning in terms of S-R bonds, but this view is inadequate unless it can be made to include central affective processes.

An environmental situation, through conditioning, comes to arouse affective processes directly. To illustrate: If a rat is placed upon a piece of apparatus, he learns to respond to the stimulus-pattern of his surroundings; but, in addition to the usual S-R patterns, the stimulus-situation produces an affective arousal. If there is a positive affective arousal, the whole situation becomes hedonically positive so that the animal comes to react positively to environmental stimulus-cues. If the situation is hedonically negative, the environmental stimulus-cues come to arouse negative affectivity—call it distress, anxiety, fear, or whatever you will.

There is definitely an internal conditioning of affective processes along with the usual conditioning described in S-R terms. By human analogy it can be said that the animal learns how to *feel* in the situation as well as what cognitive discriminations to make and what acts to perform.

7. *Affective processes regulate behavior by influencing choice.* Numerous experiments upon the development of food preferences show that the sign and intensity of affective processes influence choice. The development of a food preference between two accept-

able foods indicates which food-stimulus arouses the more intense affective process.

The acquisition of a preferential discrimination is not an instance of pure learning because affective processes determine whether one preference or its opposite will develop and, further, the relative hedonic intensities associated with two stimuli determine the rate of growth of a preferential pattern.

8. *Neurobehavioral patterns are organized according to the hedonic principle of maximizing the positive and minimizing the negative affective arousal.* This principle has a very wide range of application. It is seen most clearly in situations that involve choice. The stimulus associated with the more intense affective arousal dominates the preferential discrimination.

#### CONCLUSION

Although feelings of pleasantness and unpleasantness are known directly only in human experience, the facts of animal behavior make it necessary to postulate that affective processes have an objective existence. Since affective processes are not directly observed in behavior, they must be postulated as intervening variables. The postulate brings together in an orderly way a large and complex body of interrelated facts. Moreover, there are indications that the hypothetical construct of affective processes can some day be replaced by a physiological account of the intervening events.

Affective processes are motivational in the sense that they arouse, sustain, regulate, direct, and organize neurobehavioral patterns. Reinforcement and extinction are viewed as changes in performance dependent upon affective processes. Reinforcement must be distinguished from a change in habit strength due to exercise (practice, drill, training).

The postulate that affective processes have an objective existence is demanded by the facts of food acceptance but the postulate has a wider range of possible application—to sexual behavior, play, manipulation, and exploration, as well as to human action. Some principles of experimental hedonism have been tentatively formulated and it is suggested that they be tested in the laboratory.

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## A NOTE ON SEX DIFFERENCES IN THE DEVELOPMENT OF MASCULINE AND FEMININE IDENTIFICATION<sup>1</sup>

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The purpose of this note is to contribute to the theoretical formulation of sex differences in the development of masculine and feminine identification, and to review research findings relevant to this formulation. The concept of identification has held a prominent position not only in psychoanalysis, but also in other psychological theories (Cava & Rausch, 1952; Fenichel, 1945; Lazowick, 1955; Martin, 1954; Mowrer, 1953; Sanford, 1955; Stokes, 1950; Tiller, 1958; Tolman, 1943). Sanford said of the term "identification":

A term that can be employed in so many different ways and that, as Tolman says, has been accepted by most psychologists and sociologists, could hardly mean anything very precise. It might be proposed, quite seriously, that we give up the term "identification" altogether. . . . We must in any case specify "what kind" [of identification] . . . (Sanford, 1955, p. 107).

In this paper an attempt is made to comply with Sanford's latter suggestion rather than throw out the term "identification" altogether. Such widespread use of the term suggests, if nothing more, its potential utility with adequate clarification.

The present formulation differs from the classical Freudian position which

postulates that girls experience greater difficulty than boys in developing appropriate sexual identification because of their envy of the genital organ possessed by little boys. It also differs from the Freudian position that, because the girl has the same-sex parent (the mother) as her first love-object, she must therefore overcome a homosexual hurdle in developing same-sex identification (Fenichel, 1945). The position taken in this paper is in agreement with those who hold that, on the contrary, the early closeness of the girl to the same-sex parent (the mother) gives her an initial advantage in progressing toward appropriate identification (Brown, 1956; Mowrer, 1953). This initial advantage is thought to be counterbalanced, to a large extent, by later learning experiences in this masculine-oriented culture.

Before developing this formulation further, let us differentiate the concept of identification from other similar concepts. Brown (1956) clarified the concept of sex-role identification considerably by contrasting it to *sex-role preference*. Sex-role preference refers to the desire to adopt the behavior associated with one sex or the other, or the perception of such behavior as preferable or more desirable. This concept has been measured by simply asking Ss whether they have ever wished to be of the opposite sex (Fortune survey, 1946; Gallup, 1955; Terman, 1938). It has also been measured by having children state their preference for objects, or pictures of objects, characteristic of one sex or the other

<sup>1</sup> This article is based on a paper of the same title read at the May 1958 annual meeting of the Rocky Mountain Psychological Association in Santa Fe, New Mexico. The writer not only leaned heavily on publications by Daniel G. Brown, U. S. Air Force Academy, in developing this theoretical formulation, but he also gained a great deal from discussions with him.

(Brown: 1956, 1957; Rabban, 1950). Let us add the concept of *sex-role adoption*. This concept refers to the actual adoption of behavior characteristic of one sex or the other, not simply the desire to adopt such behavior. Women, for example, sometimes wear clothes usually associated with males, e.g., trousers. Men sometimes become beauty operators, a vocation usually associated with women. This concept refers to one's overt behavior, not to one's sex-role preference. An individual may, for example, *adopt* behavior characteristic of his own sex because it is expedient to do so, not because he *prefers* doing so. Sex-role preference is, to this extent, irrelevant to this particular concept. The sex role one actually incorporates, i.e., the role one identifies with, may, in some cases, also be irrelevant to sex-role adoption.

*Sex-role identification* is reserved to refer to the actual incorporation of the role of a given sex, and to the unconscious reactions characteristic of that role. Thus, a person may be identified with the opposite sex, but for expediency adopt much of the behavior characteristic of his own sex. He may even prefer the role of his own sex, although identified with the opposite-sex role. One would expect such a person, being identified with the opposite sex, to have many unconscious reactions characteristic of the opposite-sex role despite his adopting much of the behavior characteristic of the same-sex role. On the other hand, the woman who, on appropriate occasions, adopts aspects characteristic of the opposite-sex role, such as wearing trousers or wearing short hair, is certainly not necessarily identified with the male role. Thus, *sex-role adoption* refers to overt behavior characteristic of a given sex, and *sex-role identification* refers to a more basic process characteristic of a given sex. Sex-role identification is

much more difficult to measure than sex-role preference or adoption. Attempts have been made to measure what is here referred to as sex-role identification through projective techniques, such as human figure drawings (Brown & Tolor, 1957; Jolles, 1952; Morris, 1955; Tiller, 1958; Tolor, 1955; Weider & Noller: 1950, 1953), and through measuring the similarity between responses of parents and their children (Gray & Klaus, 1956; Lazowick, 1955).

It is probably true that most individuals may be said to prefer, adopt, and identify with their own sex role. Most psychologists associate psychological disturbances with a lack of harmony among aspects of an individual's sex role. With the present conceptual scheme a variety of combinations are theoretically possible, e.g., a person might identify with and adopt the pattern of his own sex, but still prefer the opposite-sex role. On the other hand, a person might identify with the opposite-sex role, adopt the behavior of his own sex, and also consciously prefer the same-sex role, etc. These sex-role definitions should become better clarified in the body of the paper.

#### THEORETICAL FORMULATION

Before stating specific hypotheses let us briefly formulate the position taken in this paper concerning the development of masculine and feminine identification. The developmental processes, as presented here, are not considered inevitable nor universal. If these processes are appropriately described for the U. S. culture of today, they may not fit a significantly altered U. S. culture of the future. Moreover, if these processes are appropriately described for the U. S. culture, they may, nevertheless, be inappropriate to many other cultures. Cross-cultural studies

should help verify and amplify the hypotheses presented in this paper.

First, it is assumed that the process of identification follows the laws of learning. Next, it is postulated that, for both male and female infants, learning to identify with the mother (or the person playing the mother-role) is among the earliest learning experiences. In this formulation, it is considered one of the major sex differences in the development of identification that the boy must shift from his initial identification with the mother to identification with the masculine role, whereas the girl need make no such shift.

The shift from mother to masculine identification is begun when the boy discovers that he somehow does not belong in the same sex-category as the mother, but rather as the father; that he is no longer almost completely in a woman's world characterized by the maternal care received during infancy, but is now increasingly in a man's world. It is true that in early childhood, as well as in infancy, the child's life is mainly peopled with women rather than men, but the ideology of our culture in general, and the demands made on the little boy in particular, are masculine in nature. Despite the shortage of male models, a somewhat stereotyped and conventional masculine role is nonetheless rather clearly spelled out for him. A study by Sherriffs and Jarrett (1953) indicated that men and women share the same stereotypes about the two sexes. They found that "... virtually no behavior or quality escapes inclusion in either a male or female 'stereotype,' and that these stereotypes are substantially the same whether held by men or women" (Sherriffs & Jarrett, 1953, p. 161).

If the boy behaves like a "little man," say by not crying when hurt, this "brave" behavior is reinforced. Perhaps he is rewarded by being called

"Mommy's nice little man." If, on the other hand, he does not behave in a masculine-stereotyped fashion, say he cries when hurt, this behavior may be negatively reinforced, e.g., by being called a sissy. If he behaves in a feminine-stereotyped fashion, say by playing with dolls beyond a certain age, he may be similarly ridiculed. Moreover, he is rewarded simply for having been born masculine through countless privileges accorded males but not females. The boy learns to prefer the masculine role to the feminine one, to adopt the masculine role, and, in time, to identify with it. Sex-role identification, being a more deeply rooted process than either sex-role preference or sex-role adoption, is consequently more slowly changed. However, through the reinforcement of the culture's highly developed system of rewards and punishment, the boy's early learned identification with the mother eventually weakens and becomes more or less replaced by the later learned identification with a culturally defined, somewhat stereotyped masculine role.

The development of the appropriate sex-role identification for the girl is considered, in many ways, the converse of that for the boy. When the girl leaves infancy she goes from a woman's world of mother care to a man's world. Being feminine, she thus moves from a same-sex- to an opposite-sex-oriented world, whereas the boy, conversely, moves from an opposite-sex- to a same-sex-oriented world. Unlike the situation for the boy, whose sex role is well spelled out for him, the girl, upon leaving infancy, does not receive adequate reinforcement through distinct rewards for adopting the feminine role, and definite punishment for adopting the masculine one. On the contrary, she is, in a sense, punished simply for being born female, whereas the

boy is rewarded simply for being born male. Findings in *Patterns of Child Rearing*, by Sears et al. (1957), support the suggestion that the girl is, in a way, punished for being female. The girls were found to be treated less permissively than boys and more conformity was demanded of them. Hubert and Britton (1957) also found mothers of boys to be less strict with them, expect less understanding of rules, and to allow more activity. The girl quickly learns to prefer the masculine role since our culture, despite definite changes, is still masculine-centered and masculine-oriented, and offers the male many privileges and much prestige not accorded the female. As Brown pointed out,

The superior position and privileged status of the male permeates nearly every aspect, minor and major, of our social life. The gadgets and prizes in boxes of breakfast cereal, for example, commonly have a strong masculine rather than feminine appeal. And the most basic social institutions perpetuate this pattern of masculine aggrandizement. Thus, the Judeo-Christian faiths involve worshipping God, a "Father," rather than a "Mother," and Christ, a "Son," rather than a "Daughter" (Brown, 1958, p. 235).

Smith (1939) found results to suggest that children, as they grow older, increasingly learn to give males prestige. Smith asked children from eight to 15 to vote on whether boys or girls had desirable and undesirable traits. He found: (a) with increase in age, boys have a progressively poorer relative opinion of girls, and girls have a progressively better relative opinion of boys; (b) with increase in age, boys have a progressively better opinion of themselves, and girls have a progressively poorer opinion of themselves. Kitay (1940) found that women share with men the prejudices prevailing in our culture against their own sex.

Not only does the girl learn to prefer the masculine role because of its many

advantages, but she, unlike the boy, is not given the degree of negative reinforcement for adopting certain aspects of the opposite-sex role. Although restricted in many ways more than boys, girls are nevertheless allowed more freedom than boys in opposite-sex role adoption. For a girl to be a tomboy does not involve the censure that results when a boy is a sissy. Girls may wear masculine clothing (shirts, trousers), but boys may not wear feminine clothing (skirts, dresses). Girls may play with toys typically associated with boys (cars, trucks, erector sets, guns), but boys are discouraged from playing with feminine toys (dolls, tea sets).

Data from two national sample interview studies of adolescents, reported by Douvan (1957a, 1957b), suggest that the role for the adolescent girl is very poorly defined by the culture. Since she is typically not yet married, the adolescent girl cannot play her primary role of wife and mother. Furthermore, the culture discourages her from taking action to realize this primary role. The female is not supposed to take the major initiative in choosing a mate. She must, to a large extent, be chosen as a mate rather than actively choosing. Moreover, because her primary goal is marriage and family, the girl's vocational plans do not imply the same career commitment that the boy's vocational plans imply for him. Douvan concludes that "girls . . . can do little about the central aspect of feminine identity before marriage" (1957b, p. 190).

The girl, however, has the same-sex parental model for identification (the mother) with her more than the boy has the same-sex parental model (the father) with him. Both boys and girls usually spend more time with their mothers than with their fathers. They see the mother engaging in many activities, and under many circumstances in

which they do not see the father. There is much incidental learning which takes places from such contact with the mother. Although both boys and girls doubtless learn a great deal in this incidental fashion, it is only the girls, not the boys, who can, later on at the appropriate time, apply such latent learning in a direct fashion in their lives. The boys, being separated more from their fathers than girls from their mothers, tend to identify with the stereotype of the masculine role which the culture in general, not simply the father in particular, spells out for them. The girl, on the other hand, tends to identify with aspects of her own mother's role specifically.

However, the girl is still affected by many cultural pressures despite the fact that she need not shift identification, and despite the physical presence of the mother during her development. In this formulation it is predicted that the prestige and privileges afforded males but not females, and the lack of punishment for adopting aspects of the masculine role, have a slow, corrosive, weakening effect on the girl's feminine identification. Conversely, the prestige and privileges accorded the male, the rewards offered for adopting the masculine role, and the punishment for not doing so, are predicted to have a strengthening effect on the boy's masculine identification.

#### HYPOTHESES

The following hypotheses emerge from this formulation:

1. The young boy's same-sex identification is at first not very firm because of the shift from mother to masculine identification. On the other hand, the young girl, because she need make no such shift in identification, is relatively firm in her initial feminine identification. However, the culture reinforces

the boy in developing masculine identification much more adequately than it does the girl in developing feminine identification. *Consequently, with increasing age, males become relatively more firmly identified with the masculine role and females relatively less firmly identified with the feminine role.*

2. The culture offers higher prestige and more advantages to the male than to the female. *Consequently, a larger proportion of females than males will show preference for the role of the opposite sex.*

3. Not only is the male role accorded more prestige than the female role, but boys are more likely to be punished than girls for adopting aspects of the opposite-sex role. *Therefore, a higher proportion of females than males adopt aspects of the role of the opposite sex.*

4. The girl has the same-sex parent (the mother) with her more than the boy has the same-sex parent (the father) with him as a model for identification. However, a stereotyped sort of masculine role is spelled out rather clearly for the boys by the culture. *Consequently, males tend to identify with a cultural stereotype of the masculine role, whereas females tend to identify with aspects of their own mothers' role specifically.*

#### TEST OF HYPOTHESES

Let us now see how consistently these hypotheses fit previous findings and whether this formulation helps clarify seeming contradictions.

If Hypothesis 1 is valid, that with increasing age, boys become relatively more firmly identified with the masculine role and girls relatively less firmly identified with the feminine role, and assuming that figure drawings constitute an adequate measure of identification, then this hypothesized trend should be reflected in the sex of the

figure drawn first. The data do seem, in fact, to support this hypothesis. Brown and Tolor (1957) reviewed a number of studies on human figure drawings. The studies on figure drawings with children (Jolles, 1952; Morris, 1955; Tolor & Tolor, 1955; Weider & Noller: 1950, 1953) show that, with younger children, a higher proportion of girls than boys drew the same-sex figure first, and with older children this trend is reversed, and a larger proportion of boys than girls drew the same-sex figure first. A study by Jolles (1952), using a wide age range, might be specifically cited in this regard. Jolles found that with children between five and 12, a significantly higher proportion of younger boys drew the opposite-sex figure first than did older boys. A significantly higher proportion of 11- and 12-year-old girls drew the opposite-sex figure first than did boys of the same age.

Lynn and Sawrey,<sup>2</sup> in an unpublished study in which eight- and nine-year-old Norwegian children were asked to draw a family (in contrast to drawing a person), found that a higher proportion of girls than boys drew the same-sex parent figure first, largest, and in most detail.

Despite the fact that with younger children a higher proportion of girls than boys drew the same-sex figure first, studies with adults consistently show a higher proportion of men than women drawing the same-sex figure first. Brown and Tolor (1957) combined findings from several studies of figure drawings with college Ss and found that 91% of the men drew the male figure first while only 67% of the women drew the female figure first.

Thus, the findings on figure drawings support the hypothesis that with

increasing age males become more firmly same-sex identified and females relatively less firmly same-sex identified. However, Brown and Tolor (1957) found evidence leading them to suggest that human figure drawings may be an inadequate test of identification. Confidence in the validity of this hypothesis must await substantiation through further research findings.

In this formulation it is considered one thing to show a sex-role preference, and quite another to form a sex-role identification. Hypothesis 2 predicts that, because of higher prestige and greater privileges accorded the masculine role, a higher proportion of females than males will show opposite-sex-role preference. In this connection Rabbani (1950) asked 300 children between 30 months and eight years of age to choose the toys they liked best from a number of toys. Some of the toys were judged to be typically associated with boys and others with girls. All of the Ss were also asked to pick a doll which resembled them most and to indicate the sex of the doll. In addition, they were asked whether they would like to be a "mama" or "daddy" when they grow up. The results showed no significant differences between three-year-old children, but otherwise boys showed significantly more masculine preferences than girls feminine preferences.

Brown (1957) administered the It Scale for Children to 303 boys and 310 girls between the ages of approximately 5½ and 11½. The It scale is composed of pictures of various objects and figures typical of and associated with the role of one sex in contrast to the other. A card with a child-figure drawing on it, referred to as "It," is used by having each S make choices for It. Brown found that boys showed a much stronger preference for the masculine role than girls for the fem-

<sup>2</sup> Lynn, D. B., and Sawrey, W. L. Sex differences in the personality development of Norwegian children.

inine role, particularly in all grades below the fifth. He found that girls at the kindergarten level showed a preference pattern characterized by relatively equal preference for masculine and feminine elements, and girls from the first grade through the fourth grade showed a stronger preference for the masculine role than for the feminine role. In contrast to girls in all earlier grade levels, girls in the fifth grade showed a predominant preference for the feminine role.

The Lynn Structured Doll Play Test (Lynn: 1955, 1957a, 1957b; Lynn & Lynn, in press; Lynn & Sawrey, in press; Tiller, 1958) was used in the study of 80 eight- and nine-year-old Norwegian children mentioned above in connection with an unpublished study by Lynn and Sawrey. The Structured Doll Play Test (SDP) is a projective test in which the *S* is presented with dolls representing family and peer group figures in a series of typical family and peer group situations. The *S* resolves these situations through doll play. One of the SDP situations required the *S* to choose either the boy or girl doll as the one for the ego-doll to play with. The results showed a significantly higher proportion of girls choosing the boy doll (the opposite-sex child doll) than the girl doll (the same-sex child doll). Thus, despite the fact that these same Norwegian girls had drawn the same-sex parent figure first, largest, and in most detail, they nevertheless showed a preference for the opposite-sex child doll.

These results are consistent with studies of sex-role preference in adults in which men and women were asked whether they had ever wished to belong to the opposite sex. These studies show that below 5% of adult males as contrasted to as high as 31% of adult females recall consciously having been

aware of the desire to be of the opposite sex (Fortune survey, 1946; Gallup, 1955; Terman, 1938).

Thus, the research findings in general support the hypothesis that more females prefer the masculine sex role than males the feminine role.

Hypothesis 3 predicts that more females not only prefer, but also adopt the masculine role than males do the feminine role. Emmerich (in press) used a structured doll play interview with 31 *Ss* between 3½ and 5 years of age. Emmerich measured the degree of similarity between the *S's* conception of his parent's nurturance-control attitude and the *S's* own nurturance-control attitude. The *S's* parent's nurturance-control attitude was indicated by the doll play fantasy of the parent doll's actions toward a child doll. The *S's* own nurturance-control attitude was indicated by the fantasy of the child doll's actions toward a baby doll. The degree of similarity between the parent's and *S's* attitude was the difference between the parent doll's and the child doll's nurturance-control scores. In the present conceptual framework this is considered a measure of fantasied sex-role adoption. Emmerich found that only the boys but not the girls showed a significant tendency to select the same-sex parent as a model more than the opposite-sex parent. Thus, the boys adopted (in fantasy) the father role more closely than they did the mother role. In this way the hypothesis tended to be supported, at least for young children.

As was pointed out above, the mother is typically with the children more than the father is, thus making herself available as a model for identification more frequently than the father. Largely for this reason, Hypothesis 4 predicts that males tend to identify with a cultural stereotype of the masculine role whereas females

tend to identify with aspects of their own mothers' role specifically. Gray and Klaus (1956) did a study relevant to this hypothesis, using responses to a sentence completion test and to the Allport-Vernon-Lindzey Study of Values filled out by 34 female and 28 male college students, their parents, and by the students as they believed their mothers and fathers would respond. They found much more similarity between the women and their mothers than between the men and their fathers, both as tested and as perceived.

Hypothesis 4 was also supported in a study by Lazowick (1955). The Ss in this study were 30 college students. These Ss and their mothers and fathers were required to rate concepts, e.g., "myself," "father," "mother," etc. The degree of similarity between "meanings" of each concept as rated by Ss and their parents was then determined. It was found that the similarity between fathers and their own children was not significantly greater than between fathers and children randomly matched. On the other hand, the similarity between mothers and their own children was greater than between mothers and children randomly matched.

Thus, despite the fact that data on figure drawings suggest that more men are same-sex identified than women, these results suggest that women are more closely identified with aspects of the role of their own same-sex parent (mother) specifically than men are with their own same-sex parent (father).

What are some of the ways this theoretical formulation may clarify seemingly contradictory or confusing findings? This paper reviewed studies showing that a higher proportion of girls than boys chose objects and pictures of objects characteristically considered masculine (Brown: 1956,

1957; Rabban, 1950); and yet, in the study by Lynn and Sawrey, a higher proportion of eight-year-old girls than boys drew the same-sex parent figure first, largest, and in most detail. These findings are very confusing if the term "identification" is used in connection with both the operations "sex-role object choice" and "parent drawings." The differentiation suggested by Brown (1956), and also used in this formulation, between sex-role preference and sex-role identification may eliminate the contradiction in these results. The studies of choice of masculine and feminine objects are considered, in this formulation, studies of *sex-role preference*; whereas the studies of figure drawings are considered studies of *sex-role identification*.

The results in which a higher proportion of adult males than females drew the same-sex figure first are in seeming contradiction with data reviewed showing a closer similarity between responses of women and their mothers' responses, than of men and their fathers' responses (Gray & Klaus, 1956). The contradiction is removed by the hypothesis that the male identifies with a stereotype of the masculine role, and the female with her mother's role specifically.

The data showing that females responded with more similarity to their own mothers' responses than males to their fathers' responses (Gray & Klaus, 1956) may also seem to contradict the data in the study by Emmerich (in press) in which young boys, but not girls, showed a significant tendency to select the same-sex parent as a model more than the opposite-sex parent. There is, however, a great deal of difference between the operations involved in these two studies, viz. in the S's *fantasy* of the father doll's actions (Emmerich, in press), and the *actual* responses of real fathers to the mate-

rials used in the study by Gray and Klaus (1956). In the framework of the present formulation the boys in the doll play study *adopted*, in fantasy, the father role significantly more closely than they did the mother role; whereas in the study by Gray and Klaus the adult males did not *identify* as closely with their own fathers' role as the women with their own mothers' role.

#### SUMMARY

The purpose of this note is to contribute to the theoretical formulation of sex differences in the development of masculine and feminine identification, and review research findings relevant to this formulation.

There was a differentiation made among the concepts of *sex-role preference*, *sex-role adoption*, and *sex-role identification*.

The process of identification was assumed to follow the laws of learning. Both male and female infants were hypothesized to learn to identify with the mother. Boys, but not girls, must shift from this initial identification with the mother to masculine identification. Despite the fact that the girl need not shift her identification, and despite the physical presence of the mother during her development, the girl is still affected by many cultural pressures. The prestige and privileges offered males but not females, and the lack of punishment for adopting aspects of the masculine role, are predicted to have a slow, corrosive, weakening effect on the girl's feminine identification. Conversely, the prestige and privileges accorded the male, the culture's systematic rewards for adopting the masculine role, and punishment for not doing so, strengthen the boy's masculine identification.

The following hypotheses emerged:

1. With increasing age, males become relatively more firmly identified

with the masculine role, and females relatively less firmly identified with the feminine role.

2. A larger proportion of females than males will show preference for the role of the opposite sex.

3. A higher proportion of females than males adopt aspects of the role of the opposite sex.

4. Males tend to identify with a cultural stereotype of the masculine role, whereas females tend to identify with aspects of their own mothers' role specifically.

These hypotheses were generally supported by the research findings which were reviewed. This formulation may help clarify previously confusing and seemingly contradictory data.

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## MEANING AND $m$ : CORRELATED BUT SEPARATE<sup>1</sup>

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Noble (1952) has presented a Hullian analysis of meaning and meaningfulness in which meaning is identified as  $H$ , the relationship between the stimulus word and the response word. Meaningfulness is determined by the number of different  $H$ s, i.e., a stimulus word is meaningful to the extent that it elicits many response words. In this study, Noble arrived at an index of meaning by measuring the number of word associations made to a given stimulus word in a given period of time. Noble concluded:

Thus, if one were to ask a layman what he intended by saying that "home" to him means: "family, spouse, children, friends, love," etc., he would doubtless reply, "I think of these things when 'home' is mentioned." . . . A learning theorist would explain that to the auditory (or visual)  $S$  home, these various verbal  $R$ s have become conditioned . . . and under appropriate conditions . . . are elicited. The meaning of  $S$  subsists in the  $H$ s developed to it—nothing more (Noble, 1952, p. 429).

Bousfield, Cohen, and Whitmarsh (1958) have recently presented a view of meaning which is consonant with Noble's view. They state that perception of a meaningful word involves the elicitation of two types of implicit response. First, the person says the word subvocally. This is called a verbal representational response,  $R_{rr}$ . In addition, however, the subject reacts by making another group of implicit verbal associative responses.

For example, to the word BLACK, the subject might respond WHITE, DARK, CAT, etc.

These responses may be said to comprise the associative response composite,  $R_{ra}$  comp. Under appropriate conditions the subject may produce the  $R_{ra}$  and the  $R_{ra}$  comp explicitly by saying or writing them. Though a definition of meaning is perhaps gratuitous in this discussion, we believe it is useful to identify meaning with the  $R_{ra}$  comp. This definition appears to be consistent with the Hullian interpretation presented by Noble (Bousfield, Cohen, & Whitmarsh, 1958, p. 1).

This approach contrasts with another concept of word meaning which has been developed by other psychologists, e.g., Cofer and Foley (1942), Mowrer (1954), and Osgood (1953). This interpretation, also based on Hullian concepts, states that when a word is contiguously presented with a stimulus object, part of the response elicited by the object may be stably conditioned to the word. This conditioned response becomes the meaning of the word. Osgood has indicated in detail that certain components of the total response elicited by a stimulus object are more readily conditionable (less interfering, less effortful, etc.) and hence are more likely to contribute to the final form of the representational mediation process (i.e., meaning response).

The conditioning of a meaning response can also take place through higher-order conditioning, in which case the US, the stimulus object, is replaced by a word which through prior conditioning already elicits a meaning response. Both of these processes are schematized in Fig. 1. In the upper part of the figure the word BAD is

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paired with a punishing stimulus. The punishment elicits a number of responses which may be called "unpleasant," or of "negative value." After BAD has been paired with punishment a number of times, the conditionable responses (called "detachable" by Osgood) elicited by the punishment are conditioned to BAD. These responses come to constitute the stable meaning of BAD.

Assuming that the conditioning has been sufficiently strong, contiguous presentation of BAD and another word which has no meaning would result in higher-order conditioning. That is, the meaning response elicited by BAD would be conditioned to some extent to the new CS-word. In an actual life situation, a child might be told "Evil means bad." The negative meaning elicited by BAD would be conditioned to EVIL. This is depicted in Part *b* of Fig. 1.

Recently Osgood, Suci, and Tannenbaum (1957) have taken issue with Noble's approach to meaning. They would accept *m* as a measure of the association value of a stimulus word, but vehemently reject the interpretation that the associations may be thought of as word meaning. They state that a basic distinction exists between the meaning of a sign and its associations. They continue as follows:

This point needs to be labored because one recent writer (Noble, 1952), at least, has seriously proposed that the meaning of a sign is nothing more than the number of different associations between it as a stimulus and other signs as responses. According to Noble, "The index of meaning (*m*) of a particular stimulus was defined . . . as the grand mean number of (acceptable) written responses given by all Ss within a 60 sec. period." . . . It is his basic notion—that meaning and association can be equated—which is wrong. Does BLACK mean *white* because this is the most common associate? Does NEEDLE mean *sew*, BREAD mean *butter*, MAN mean *woman*? Noble's *m* might be identified as meaningfulness rather

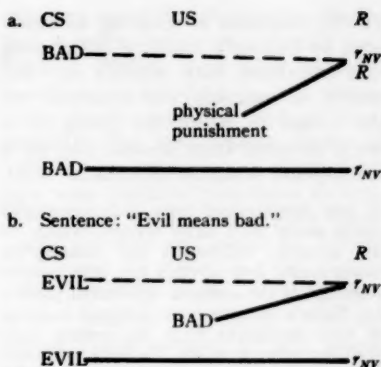


FIG. 1. Diagram a depicts first-order conditioning of word meaning. After a number of pairings of BAD, the CS, with punishment, the US, BAD comes to elicit the conditionable (i.e., "detachable") components of the responses elicited by the punishment (symbolized as  $r_{NV}$  because of the negative value). The components of the total response which are not stably conditioned are symbolized as  $r_{NY}$ . Diagram b depicts second-order conditioning of meaning. The negative value meaning responses now elicited by BAD are conditioned to EVIL through contiguous presentation of the two words in the sentence. Although not schematized, it is understood that  $r_{NV}$ , as a mediating response, may through the stimuli it produces elicit other overt or implicit responses.

than meaning, or better, simply the association value of the stimulus, since this is actually what he is measuring (Osgood, Suci, & Tannenbaum, 1957, pp. 16-17).

Thus, a rather sharp clash of interpretations is enjoined.

#### RELATIONSHIP BETWEEN THE TWO MEASURES OF MEANING

Osgood et al. insist that meaning of a word is not the same as the word's verbal associates, i.e., the meaning of a word involves a psychological process which is separate from word association processes. However, two recently reported experimental results have shown a relationship between *m* and semantic measures. In the first study, reported by Jenkins and Russell

(1956), intensity of meaning, as measured by Osgood's semantic differential, was correlated with Noble's *m*. Intensity of meaning was measured by the extent to which the rating of a word deviated from neutral (4) on a seven-point semantic differential scale.

It was hypothesized here that meaningful words would elicit many extreme ratings on the semantic differential and meaningless words would tend to elicit few such ratings. Accordingly the semantic differential profiles for Noble's concepts were analyzed in terms of their deviations from the neutral scale positions (that is to say, *D*s were calculated for each profile against a hypothetical profile running through the middle of each scale). The hypothesis was in general well substantiated. The correlation between the size of *D* and Noble's "*m*" was +.71. This represents the first connecting link between what seemed at the outset to be two entirely different ways of talking about psychological meaning (Jenkins & Russell, 1956, p. 7).

In addition, Noble (1958) has recently presented evidence which supports this result. In his experiment, *m* was correlated with what may be called a measure of intensity of evaluative meaning, one of the main factors of meaning found by Osgood and Suci (1955). Instead of using a semantic differential scale, Noble had the *S*s rate each word neutral, pleasant, unpleasant, or mixed. A semantic differential scale of pleasant-unpleasant would allow 7 gradations of evaluative meaning, and has no "mixed" category. Nevertheless, Noble's procedure seems to measure evaluative meaning, and the results should be roughly comparable to scoring words on a pleasant-unpleasant semantic differential scale.

Noble found in his study that this type of evaluative rating (which he calls a measure of emotionality) was correlated with *m* .57 ( $p = .001$  level of significance). Thus, in this study and the study of Jenkins and Russell, larger *m* measures were associated with more intense meaning.

The question arises, especially in view of the rejection by Osgood et al. of the relevance of Noble's approach to a conception of meaning, of how to account for the relationship of Noble's measure of meaning and Osgood's semantic measures. As Jenkins and Russell imply, the two studies indicate a relationship which demands explanation—if meaning and word associations are to be separately considered. The present paper, in distinguishing meaning and *m*, must explain the relationship between the two. This explanation will also indicate why a word's meaning may be confused with the word's direct verbal associations.

#### BASIS OF THE RELATIONSHIP BETWEEN *m* AND MEANING

In the conditioning of word meaning it seems likely that both primary and higher-order conditioning are important, i.e., a word gets its meaning through both of the processes represented in Fig. 1. To some extent word meaning is obtained, and maintained, by contiguous presentation with certain primary stimulus objects. In addition, it is suggested that word meaning is conditioned through contiguous presentations with other words. It is this latter occurrence which accounts for the correlation between intensity of meaning and *m* and which, it is thought, results in confusing the two independent psychological processes. A stimulus word gets its meaning, in part, because each time it is paired with another word the meaning of the response word is conditioned to the stimulus word. This also strengthens the associations between stimulus word and response words. For example, if a nonsense syllable was paired with the word "bad" a number of times, direct associations between the syllable and "bad" would be formed. In addition, the meaning response elicited by

"bad" would be conditioned to the syllable. Because of this parallelism in development it is easy to consider direct associations, which underlie *m*, and meaning as the same thing. Thus, although *m* and meaning are not the same, the more often a word is paired with its common associates, the stronger become the direct word-word associates and the word-meaning associates, i.e., the same operation strengthens both types of associations.

The stronger the direct associations, the less the latency of response when the stimulus word is presented. Thus, in a given period of time, stronger associations will result in the occurrence of more response words, i.e., the *m* measure will be high when word associates are elicited. In addition, strong word-meaning associations will result in semantic differential ratings which are extreme.

However, if a stimulus word obtains its meaning in part from its word associates, the meaning of a stimulus word must be directly related to the meaning of its response words. Thus, a positive correlation between the meaning of stimulus words and the meaning of their word associates would support the preceding interpretation. To verify the point, the following demonstration was conducted.

# SIMILARITY OF THE MEANING OF STIMULUS-WORDS AND THE MEANING OF THEIR WORD-ASSOCIATES

Forty-six students in introductory sociology participated in the experiment.

In the experiment words were required on which semantic differential information was available, as well as information concerning the associates of the words. Jenkins, Russell, and Suci (1957) have provided data on the semantic profiles of 360 words, using

different semantic differential scales for the semantic measures. Russell and Jenkins (1954) have also provided word association data on 100 words. Some words appear in both of these studies. For the present study 10 words were chosen on which both data were available from these sources. The criteria for choosing the ten words were: (a) that the words be distributed along the "good-bad" evaluative scale, including words which had extreme positive evaluative meaning (mean rating toward 1) and words which had extreme negative evaluative meaning (mean rating toward 7), as well as words which were in between the extremes; (b) that word association data be available for the ten words selected. The words chosen are as follows: MUSIC, SWEET, TABLE, MOUNTAIN, DEEP, HARD, ROUGH, ANGER, TROUBLE, SICKNESS.

A folder was prepared which included these ten words and 40 additional words of unsystematic meaning, all arranged in random order. Under each word was a semantic differential scale of good-bad. An example is given below.

MUSIC

good : — : — : — : — : — : — : — : bad

The *Ss* were instructed in rating the meaning of the words.

Three weeks later the same Ss rated the meaning of the first twenty word associates of each of the above ten words. The twenty word associates were obtained from Russell and Jenkins (1954). In cases where a word was the associate of more than one of the "evaluative" words it was only listed once. Thus, a folder was used which included the 172 resulting words arranged in random order. The format of this folder was the same as with the first rating task: each word was listed

TABLE 1  
MEAN RATINGS OF STIMULUS WORDS AND  
TWENTY ASSOCIATED WORDS

Word	Minnesota	Arizona State	20 Associates (AS)
Music	1.60	1.56	2.47
Sweet	1.93	1.98	2.74
Table	2.37	2.85	2.75
Mountain	2.73	2.32	2.96
Deep	3.77	3.85	3.20
Hard	4.13	3.91	3.24
Rough	5.00	4.79	3.11
Anger	5.57	5.88	4.50
Trouble	6.10	6.39	4.28
Sickness	6.30	6.56	3.99

Note.—On the scales, 1 was "good" and 7 "bad."

with a "good-bad" scale beneath it. The same instructions were used in administration.

In the analysis of the results the mean meaning scores for the original ten words were computed. In addition, the mean meaning score for the 20 associates of each of the ten words was computed. These means are listed in Table 1. The table also includes the mean meaning scores (on good-bad) on the same ten words obtained by Jenkins, Russell, and Suci using University of Minnesota students.

A rank order correlation coefficient was computed between the mean meaning score obtained in this study and the mean meaning scores obtained by Jenkins, Russell, and Suci. The correlation was .99.

The extent of the relationship between the meaning of the stimulus words and the meaning of their word associates was also measured by rank order correlation of their respective mean evaluative meaning scores. The rank order correlation coefficient was .90, which is significant at better than the .01 level. The results support the hypothesis that the meaning of the associates of a stimulus word tends to

be the same as the meaning of the word. This gives credence to the hypothesis that *m* and intensity of meaning are correlated because associating two words conditions the meaning response of one to the other in addition to strengthening associations between the words.

The more often the stimulus word is paired with its associates, the stronger will the direct associations become. At the same time, the meaning of the associates will be more strongly conditioned to the stimulus word, i.e., the stimulus word will acquire more intense meaning. (Studies to be discussed in the next section will give additional support to this interpretation.) Other things equal, contiguous presentations of words will strengthen the associations responsible for high *m* as well as the associations responsible for intense semantic differential meaning. This effect should not necessarily depend on the frequency of use of a word. It depends on the frequency with which a word is paired with a group of associates which has a certain type of meaning. Words such as "the" no doubt occur extremely frequently with few repeats of any particular word associate. Because of this, "the" would have a great many weak word associates, with many different and probably antagonistic meanings. For this reason it would be thought that "the" would not have strong word associates, nor would it elicit strong meaning responses.

#### ADDITIONAL DISTINCTIONS IN THE CONCEPTS OF MEANING

The foregoing interpretations and empirical results argue that the verbal associates which words elicit do not account for word meaning. The confusion between these two processes may have arisen because the same operation, paired presentations of words,

strengthens both types of associations. However, if word associations and word meaning are really independent processes, it should be possible to find independent operations for their development.

In a recent experiment (A. W. Staats & C. K. Staats, 1958b), a GSR was conditioned to the word LARGE by pairing the word with noxious stimulation (shock or loud noise—adjusted to be unpleasant for each *S*) as it was presented in a list of words to be learned. After the conditioning the meaning of the word was measured on the semantic differential scale of pleasant-unpleasant. According to the theory of meaning already discussed, part of the “negative” response elicited by the noxious stimuli should be conditioned to LARGE (in the same manner as the GSR) and become, in part, the meaning of the word. This “negative” conditioned meaning response should then mediate the negative rating of LARGE on the semantic differential. The prediction was substantiated—negative evaluative meaning was conditioned to LARGE, without pairing it with other words. In addition, there was a significant correlation between the intensity of the conditioned GSR and the intensity of the conditioned negative evaluative meaning response. There is thus a suggestion that the conditioned GSR and meaning response were part of the same process. Osgood describes an example of the conditioning of negative evaluative meaning which is analogous to this interpretation. In his example, the word SPIDER is paired with the object, a spider, and some of the responses elicited by the spider, including autonomic responses of an aversive nature, are conditioned to the sign. He describes the conditioned autonomic responses as those which “literally confer the unpleasant, connotative meaning of threat

upon this word” (Osgood, 1953, p. 696). It may be concluded from the above experimental results that meaning can be conditioned to a word through systematically pairing the word with a nonverbal aspect of the environment.

It might be suggested, however, that nonverbal objects on presentation elicit implicit verbal responses in the individual. Assuming this to be the case in this experimental procedure, if each time shock or sound was presented the *S* said “shock” or “sound,” then word associations to LARGE could be formed. Then it might be said that LARGE had gained negative evaluative meaning because it later elicited the words “shock” and “sound.” (This, of course, leaves unanswered the question of how “shock” and “sound” have acquired negative meaning.) However, this interpretation does not account for the significant correlation between the intensity of the conditioned GSR and the intensity of the conditioned meaning response. In addition, it seems questionable whether electric shock and an unfamiliar sound, especially in the experimental procedure, would elicit implicit naming responses.

The above experiment was thought to illustrate first-order conditioning of meaning as depicted in the upper part of Fig. 1. In addition, however, several recent studies have shown that meaning may be conditioned through higher-order conditioning (lower part of Fig. 1) independent of strengthening word-word associations. In these experiments a visually presented verbal CS was paired *once each* with 18 *different* auditorily presented words, each word having, however, an identical component of meaning. For example, a nonsense syllable was paired with 18 words like HAPPY, PRETTY, DINER, and SWEET, which all have

what may be called a positive evaluative meaning; and another syllable was paired with words like UGLY, THIEF, AGONY, and DISGUSTING, which have negative evaluative meaning. In addition, four other syllables were each paired in the same manner with 18 different words of no special meaning—yielding a procedure involving six nonsense syllables and 108 different words. Pairing a nonsense syllable only once each with 18 words would yield 18 direct syllable-word associations, all of them weak and mutually inhibitory. On the other hand, the evaluative meaning (either positive or negative) elicited by *each* of the US-words should be strongly conditioned to the syllable with which they were paired. The expected conditioning occurred, even when Ss who were aware of the systematic pairing of a certain type of word with a nonsense syllable were excluded from the analysis. Meaning, as measured by a semantic differential scale, was conditioned to nonsense syllables (Staats, Staats, Heard, & Nims, 1959; C. K. Staats & A. W. Staats, 1957), national and proper names (A. W. Staats & C. K. Staats, 1958a), and meaningful words (Staats, Staats, & Biggs, 1958). In one experiment (C. K. Staats & A. W. Staats, 1957) the evaluative, potency, and activity factors of meaning found by Osgood and Suci (1955) were conditioned. A further study of "language conditioning" has indicated that the strength, or intensity, of the conditioned meaning increased as did the number of trials, i.e., number of syllable-meaning pairings (C. K. Staats & A. W. Staats, 1958), again with a syllable paired only once with any particular word.

Conversely, it should be possible to establish strong direct word-word associations without establishing strong word-meaning associations.

This would be possible by selecting US-words (or response-words) which elicit antagonistic meaning responses, e.g., some US-words which elicit positive evaluative meaning and an equal number which elicit negative evaluative meaning. In this case the CS-word (or stimulus word) would be paired with each US-word many times. The result would be strong direct associations between the CS- and US-words, but the meaning of the CS-word should remain neutral, as measured on a semantic differential scale. The positive conditioning would cancel the negative. Similarly, a CS-word could be paired many times with words of neutral meaning (or with nonsense syllables) so that strong direct word-word associations would result. The CS-word would then have a high *m*, but it would elicit no meaning response, or the meaning response would be neutral as measured on a semantic differential.<sup>2</sup>

The foregoing illuminates a weakness of the interpretation that word meaning is comprised of the word's verbal associates. The conception that word meaning consists of word associates makes no provision for differentiating words in terms of their relationship to different aspects of the environment. While it seems reasonable to state that a word has meaning because it has certain verbal associates, it is reasonable because the associates of the word are *meaningful* themselves. The conception becomes unreasonable

<sup>2</sup> It should be pointed out that words which parallel these examples probably exist in our language, i.e., words with strong verbal associates and weak or neutral meaning; and the opposite, words with strong meaning and few or weak verbal associates. If words such as these were included in a study measuring the relationship of *m* and intensity of meaning (such as those of Noble or Jenkins and Russell), they would tend to reduce the correlation. Perhaps the less than perfect correlations obtained in the two studies reflect this.

if the case is considered where the verbal associates are meaningless words, i.e., nonsense syllables. To state that words gain their meaning by eliciting other words leaves all words in the status of nonsense syllables which have been widely associated with one another. It is unreasonable that this process produces meaningful words. No matter how many meaningless associates a nonsense syllable has it will remain meaningless. An illustration of this can be found by imagining a person learning a foreign language solely by pairing (or defining) the foreign words with each other—without ever pairing a foreign word with aspects of the nonverbal environment, or with meaningful words in a familiar language. With sufficient practice the person could in this manner learn many word associations in the foreign language—but the new language would be meaningless.

A more complete conception of meaning than the theory of word associates must include reference to the systematic pairings of verbal stimuli and various aspects of the environment, and to the properties acquired by the verbal stimuli as a result of this process.

#### VERBAL GENERALIZATION AND MEANING

Since Bousfield et al. consider meaning to be the composite of associates of a word, they were able to deduce certain hypotheses concerning semantic generalization. A number of studies, summarized elsewhere (Cofer & Foley, 1942; Osgood, 1953), have shown that a response conditioned to a word will generalize to a word of the same or similar meaning. Bousfield et al. conclude that this generalization is mediated, at least in part, by the partial identity of the word associates of two words. They have also provided evidence demonstrating a relationship be-

tween the amount of generalization and the degree of identity of word associates. The results of the present study, however, indicate that this relationship may also reflect another variable. Since the word associates in the composites are themselves meaningful words, we may say that they too elicit meaning responses. When two response composites are similar, the meaning responses elicited by the response composites will be similar to one another. Thus, the generalization can also be considered to be due to the mediation of the common meaning responses of the common associates. Including this alternative, semantic generalization may take place on the basis of: (a) similarity of meaning responses elicited by two words, (b) similarity of the word associates elicited by two words, and (c) similarity of the meaning responses elicited by the word associates of the two words. In any situation it may be that each of these factors is contributing to the total generalization.

#### SUMMARY

Two approaches to meaning were summarized and contrasted. Word meaning may be considered to be the verbal responses made to the word, or word meaning may be a conditioned mediating response, part of the response elicited by the object denoted by the word. The present paper described the latter as meaning and distinguished word meaning from a word's verbal associates. The correlation between intensity of meaning and verbal associate measures which has been reported was seen to be a result of the fact that the same operation strengthens both: the more often a word is paired with its word associates, the stronger the connections between them. In addition, the meaning of the associates is conditioned to the word. This view

was supported by showing that the associates of a word tend to have the same meaning as the word.

The two approaches to meaning were discussed further, and it was concluded that words could not gain meaning through verbal associations per se. Originally, it is through systematically pairing words with aspects of the environment that their meaning is gained. The meaning acquired in this process may later be conditioned to other words.

It was also concluded that semantic generalization may be a function of (a) similarity of meaning between words, (b) similarity of word associates elicited by words, and (c) similarity of meaning responses elicited by word associates of words.

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